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NOTICE!!

Until September 1, 1991 manuscripts to be submitted to the *Review* should be sent to the present managing editor, Samuel C. Patterson.

Effective September 1, 1991 all new manuscripts should be submitted to the new managing editor, Professor G. Bingham Powell, Department of Political Science, University of Rochester, Rochester, New York 14627.

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ARTICLES

SOLIDARNOŚĆ: RELUCTANT VANGUARD OR MAKESHIFT COALITION?

BARBARA HEYNS
New York University
IRENEUSZ BIALECKI
Polish Academy of Science

We analyze the election data from the first "almost free" political contest in the Eastern bloc, the Polish election of June 1989. Voting data for state socialist societies provide a novel source of information on the political transitions in process in East Central Europe; the source of electoral support for opposition candidates in Poland affords a glimpse of the emerging political groups that must deal with continuing economic crises while attempting to reconstruct or consolidate democratic procedures. Data on turnout and on the ecological patterning of votes for Solidarność and for the government coalition are reviewed. Electoral victory has transformed Solidarność from an opposition movement with strong trade union roots to a political coalition with a rather different constituency: the strongest relative support for Solidarność candidates was found in rural areas, particularly in the southeast, rather than in the urban, industrial centers where the movement was born.

The Polish election in the summer of 1989 represented a stunning defeat for the communist regime and a historic moment for the Polish people. Although events in the region since the summer of 1989 have escalated, the international media were transfixed by the extraordinary spectacle of a peaceful, democratic referendum that led to the defeat of the party that had ruled Poland for more than 40 years. Even the Poles seemed astonished by the magnitude of support expressed for Solidarność. This election, even in the short term, has implications for economic and political reform in Eastern, or rather Central, Europe. The election influenced the calculations of political leaders in Poland and elsewhere as well as the forms that will be taken by future governments in Poland. The resurgence of Solidarność was sudden and, outside Poland at least, unexpected; the nature of the internal alliances and cleavages characteristic of the rekindled movement are not yet well understood, even in Poland. A new social contract has recast the role of Solidarność from a social movement representing Polish workers and opposition to a national political force.

We shall trace the political and organizational metamorphosis of Solidarność and review the election results that transformed a social movement into a governing coalition. Although a good deal has been written about the emergence of Solidarność (Arato 1981; Ascherson 1982; Ash 1983; Bakuniak and Nowak 1987; Bialecki 1982; Holzer 1986; Kuczynski and Nowak 1988; Mason 1985; Ost 1990;

Staniszkis 1984; Touraine 1983) and the aftermath of martial law (Koralewicz, Bialecki, and Watson 1987; Reiquam 1988; Swidlicki 1988), far less is known about what forms of political activity are currently feasible or likely. Instituting new electoral procedures, whether by popular referendum or party caucus, appears to be a fundamental part of democratization in state socialist societies. Although the path leading to an electoral coup in Poland may be unique, we will review in some detail the process through which an opposition movement became a ruling coalition. While Hungary has taken the lead in economic change, Poland has surely been in the forefront of political transitions (Manchin and Szelenyi 1985, 1987; Szelenyi 1983). An examination of the electoral results in the first quasi-democratic referendum in the Eastern bloc can clarify the social bases of opposition and the issues at stake for all of Central Europe in the future.

This study is based on the election returns published in the summer of 1989 in *Gazeta Wyborcza* and in *Tygodnik Solidarność*, statistical data available for the electoral units (voivodships), and survey results from the 1987 Social Structure and Mobility Project sponsored by the Polish Academy of Science. Tabulations of electoral outcomes by individual voivodships are given in the Appendix, Table A-1. In addition, interviews with Polish activists and experts were conducted in the summer of 1989 in Warsaw and Krakow.

We shall present an overview of the events that gave rise to Solidarność and precipitated the transformation from a social movement to a governing coalition. We shall argue that distinctive organizational forms as well as forms of political activity have characterized Solidarność, culminating in the electoral victory of 1989. The voting patterns indicate how broadly based the coalition of support is and how much it has changed since the beginning of the movement.

The Legacy of the Past

Solidarność represents, both symbolically and in actuality, several competing organizational forms. Three distinctive forms have emerged sequentially, although elements of each are evident in each phase of development. First, Solidarność is a trade union, the first independent, self-governing trade union in the Soviet bloc. The origins of Solidarność can be traced to the activities of workers in the shipyards of Gdansk and in the coal mines of Silesia. The novel aspect of Solidarność, however, was the broadly political nature of workers' demands; in addition to conventional concerns regarding wages or the price of bread and sausage, workers insisted that democratic institutions and political reform were needed as well. Strike committees in major industries refused to accept the economic concessions with which the government had bought labor harmony in the past. Negotiations over the registration of Solidarność in 1980 rapidly escalated into demands for official recognition of several affiliated groups, such as Rural Solidarność, a union of private farmers, and the NZS, an independent student union (Wankel 1989).

Under state socialism, economic problems are, by definition, political problems as well. When the distribution of goods—and the income needed to purchase them—are centrally controlled, the government is thereby held responsible for the total quality of social life. Moreover, the socialist state apparatus has a more difficult time opposing organizations and strikes by workers than other forms of social protest. What emerged was a nationwide social movement that embraced the trade union as the paramount symbol of the opposition. Solidarność forged an alliance between striking workers and intellectuals that was unique in Eastern Europe. Although a general strike did not materialize in 1980–81, the threat posed by the vitality of this social movement, culturally and socially, led to the imposition of martial law and the suppression of Solidarność.

Labor unrest and the round table talks that resulted precipitated a new phase for Solidarność. In the wake of several wildcat strikes in the spring of 1988, the government became convinced that it could not govern or deal with the continuing economic crisis without a broader base of legitimacy. It was increasingly obvious that badly needed economic reforms could be implemented only with greater popular backing and the support of the workers. Although Solidarność had neither organized nor anticipated the strikes that occurred in the spring of 1988, the labor unrest coupled with the fact that the union stood to regain legal status, made Solidarność the obvious choice to represent the opposition. Moreover, by 1988 the "geopolitical imperatives" had changed. The party hard line, that a military solution was the only means of salvaging national autonomy and avoiding Soviet intervention, seemed increasingly lame.

After protracted delay, the round table talks began in February 1989. For the next two months, with full national television coverage, government officials and Solidarność members deliberated the fate of the trade union and the nation. Polish citizens, many for the first time in their lives, were treated to ringside seats at the debates (or, perhaps better, *negotiations*) that were to lead to the first "almost free" election in postwar Poland and in the Eastern bloc. During this period, Solidarność began to display the characteristics of a political organization. Despite repeated assertions by the leadership that they had no political ambitions, as an organization Solidarność had ceased to act strictly as a "self-limiting" revolution.

The most immediate issue at the round table was to negotiate the terms of a general election. The government agreed to permit Solidarność to enter candidates competing for 35% of the seats in the Sejm, the traditional Polish parliament; a second body, the Senat, was to be recreated by an unrestricted popular election. Although without full legislative powers, this Senat could not be dominated by the party. A two-thirds vote of the Sejm would be required to override a veto by the Senat; in other words, if united, the opposition could prevail. Believing that an early election would be to their advantage, the government set the date for early June, barely one month after the end of the round table. The government surmised, not unreasonably, that Solidarność did not have the funds, the expertise, or the time needed to launch a sizable political campaign.

Despite the limitations of time and resources, and in the midst of accusations that foreign money and influence were instrumental, Solidarność managed to blanket both the cities and the countryside with posters, information, slogans, theme songs, and photographs and interviews with Walesa. In Poland, and perhaps the world at large, Lech Walesa is surely the second-most-famous Pole, after Pope John Paul II. The selection of candidates to stand for election, for example, was not a particularly democratic process. Nominally, the election was organized by the Solidarność civic committees under the chairmanship of Bronislaw Geremek; but Walesa personally played a major role in the selection process. Had there been more time to plan and to wage the campaign, in fact, there might have been more criticism of the slightly heavy-handed manner in which these civic committees operated and candidates were chosen.

The compromises reached at the round table regarding electoral procedures established a complicated balance of power and possibilities for the government, as well as an inordinately complex scheme for balloting. The 460 seats in the

Sejm would be divided between a slate of 299 candidates (65%) chosen by the Polish United Workers party (PZPR) and their allies in the ruling coalition and 161 independent or opposition candidates. These 161 seats would be filled democratically, by open contests, in which Solidarność (or any group or individual gathering the requisite three thousand signatures) could run for election. The upper house, the Senat, would have one hundred seats, to be elected entirely by popular vote in each of the 49 administrative districts (voivodships). Each voivodship would elect two senators, with the exception of the two largest regions (that include Warsaw and Katowice), which would elect three each.

Formally, the election was highly decentralized. Only one slate of candidates, which included Prime Minister Mieczyslaw Rakowski and 34 other members of the politburo, or governing cabinet, would seek endorsement nationwide. This "national list" was not intended to be a true contest or race; the right of the PZPR and the current government to rule was not in question. To retain power, the party elite required merely the passive approval of a simple majority of the electorate. In order to oppose the governing coalition, voters had to strike out some or all of the names on this list. By allowing their names to be placed on the ballot at all, however, the members of the government allowed the people to express their attitudes toward the communist leadership, martial law, and faltering policies. As a consequence, one of the more liberal and reformist regimes in postwar Poland suffered a crushing defeat.

In theory, the government should have been far better prepared to wage a political campaign than were the ad hoc civic committees set up by Solidarność, even with the organizational assistance provided by local parishes. The government controlled virtually all of the country's resources, including the press, the media, and other propaganda organs. But whether through complacency, bad planning, or the atrophy of appropriate political skills, the government lost badly on all fronts. The political strategy, the campaign, and the election were impressive triumphs for Solidarność. Ninety-nine of the 100 seats in the Senat were won by Solidarność; Solidarność candidates won all of the 161 seats in the Sejm available to them; and, most devastating to the government, 33 of the 35 unopposed candidates on the national list failed to win majority endorsement in the first round.

Each of the three models that are key to an understanding of the Solidarność victory and of the current political situation in Poland can be observed during the round table negotiations and in the subsequent election. Solidarność, living up to its name, stood by several activists most repugnant to the regime, insisting that they be included in the round table.[1] As a trade union movement, Solidarność avoided serious discussion of impending economic reforms, advocating instead an indexation of state salaries relative to inflation. Indeed, the round table talks were conspicuously silent regarding remedies for the economic crisis. One well-known sociologist described the proceedings as "a democratic ballet on the decks of the Titanic."[2] Neither the government nor the trade union put forward realistic proposals to reduce the enormous inefficiencies in industry or to curtail shortages. Although the political rhetoric of the round table was often confrontational, on the issue of sustaining heavy industry, the economic interests of the government and the trade union were not at odds.

As a social movement, Solidarność relied on the charisma of Lech Walesa, the spiritual authority of the church, and the democratic ideals of the Polish opposition. Between 1981 and 1989, Solidarność had managed to sustain a rudimentary underground organization, despite the lack of both official and legal standing.

Although not well developed ideologically, Solidarność has enormous appeal as a social movement. Throughout the campaign, the symbolic trappings were clearly in evidence: the Solidarność logo, posters and statesmanlike declarations by Walesa, and other campaign paraphernalia that seemed to materialize overnight. Substantial intellectual support for Solidarność undoubtedly came from the intelligentsia (Curry and Preibisz 1984), while in many voivodships the church provided space and critical organizational support.

As a political organization, Solidarność acted quickly and decisively to broaden its constituency, rally support, and mobilize resources from all fronts. While Solidarność's status as an opposition organization was well established, during the course of the campaign Solidarność claimed and successfully appropriated the entire antigovernment terrain. Although the leaders of Solidarność acknowledged that they were neither able nor prepared to rule the country, this was, curiously, never perceived as an impediment to electoral victory. Ironically, the legacy of this wildly successful first election was a crisis of legitimacy for the leadership and the ultimate collapse of the party that had allowed it to happen. The legal quandary regarding the structure of political authority in the country has yet to be fully resolved.

Electoral Support for Solidarność

The fundamental point to be made about this election is that support for Solidarność candidates cannot be separated from antigovernment sentiments in general. Two outcomes of the election are central to this conclusion: (1) the votes for Solidarność candidates as a proportion of all votes cast in a particular voivodship and (2) the proportion supporting (or, rather, not opposing) the national list. Recall that in order to register opposition to the government, voters had to cross out some or all of the names included on the national list. A clear majority (53%) rejected most of the names; only two of 35 candidates received the requisite majority.[3]

The number of votes for particular candidates on the national list tended to reflect their relative notoriety in Poland and to some extent their support for reform; however, the general tendency to accept or reject the complete national list clearly overshadowed the attributes of individual candidates. Even Walesa's widely quoted conciliatory statement that he intended to vote against only one or two of the candidates on the national list, carried little weight in the pattern of actual voting. In the aggregate, votes favoring the national list ranged from 32.2% to a high of 62.3%; more than half of the voivodships rejected all 35 candidates.

The election clearly revealed the degree to which Solidarność had captured, or recaptured, the opposition banner; it also revealed the polarization between government supporters and Solidarność supporters. The correlation between the average proportion supporting Solidarność candidates and the proportion not rejecting the national list was −.76 across voivodships.[4] In predicting the Solidarność vote across all voivodships, only one other variable (the total number of candidates running for office) ever made a significant independent contribution to explaining the strength of the Solidarność vote after the proportion supporting the national list had been entered; over half (58%) of the variance in these two electoral outcomes is shared. The residuals from an equation predicting the strength of the vote for Solidarność that included both the proportion of voters opposed to the national list and the number of candidates per seat were not significantly related to any other variable considered. Measures of the socioeconomic composition of the voivodship (e.g., the proportion of voters who were state workers, independent farmers, or pensioners and the proportion possessing a university education) load on the variance common to these two variables, rather than independently. Clearly, one cannot understand the sources of support for Solidarność without considering general attitudes toward the government in power. The electoral success of Solidarność depended profoundly on mobilizing the broad base of animosity toward the government.

The equation predicting the average proportion of votes for Solidarność candidates (S_i) from the proportion voting for the national list (N_i) and the number of candidates (C_i) on the ballot is, in metric form,[5]

$$S = 113.3 - .863N - 1.330C,$$

with t-scores of 8.98 and 3.25, respectively, and an adjusted R-squared of .66. As the model indicates, the proportion of voters supporting Solidarność candidates is a function of the proportion rejecting the national list and the number of candidates running per seat. The Solidarność civic committees designated only one candidate for each available seat; hence, the more candidates appearing on the ballot, the less likely were Solidarność nominees to receive the full count of votes. In all other respects, however, the ecological patterning of votes suggests that the strength of Solidarność is directly and strongly related to the degree of antigovernment sentiment in each voivodship.

Voter Turnout

Concepts derived from electoral politics, such as turnout, are rarely invoked in discussions of state socialism; voting studies seem irrelevant in the context of limited political choice and mandatory suffrage. Since 1948, elections in Poland have resembled those in other socialist states; the process is a ritual of legitimation, rather than a contest for power (Ekiert 1988; Flis 1988; Wiatr 1962). Elections were conventionally considered a sham, a necessary act of compliance with official pressure and propaganda; until the early 1980s, voting was virtually universal. Wiatr (1962) argued that the system in Poland evolved from "safe" to "consent" elections during the 1950s with the introduction of secret balloting; as in this election, the populace was allowed to abstain, to endorse candidates, or to delete some or all of the names.

In prior years, official efforts to stimulate voting were common. Food or other scarce consumer goods were often available near voting stations; if incentives failed, it was widely understood that nonvoting could jeopardize a bonus or a passport. Polish elections, including the most recent, are held on Sunday; in rural areas the party often provided special transportation to the polling places after mass. Even with such inducements, however, the data on electoral turnout are suspect; vote counts were often inflated by officials.

Nonvoting in state socialism, however rare, has been argued to be an act of opposition and hostility, rather than apathy or indifference (Karklins 1986; Hermet, Rose, and Rouquie 1978). In both Hungary and Poland, voting abstention has increased in recent years, despite larger numbers of candidates and more fervent government appeals; in Poland, the number of persons voting declined substantially between 1980 and 1985, from 99% to less than 60% (Ekiert 1988). Although an illicit organization at the time, Solidarność called for a boycott of the elections of 1984 and 1985 and then monitored the procedures. Whether the boycott reduced voter participation or the fact of observers reduced fraudulent methods of meeting voting quotas, official statistics reported declines in voting of as much as 25% (Ekiert 1988). Solidarność estimates of the actual turnout were even lower than official statistics.

As the date for this critical election approached, the government-dominated media began to downplay the campaign and to question the legitimacy of both the upcoming vote and the various contenders. Until the very last day, however, official sources claimed that the government's triumph was imminent and inevitable. Until the government had officially recognized and formally accepted defeat, celebration of the Solidarność victory was rather muted.

The first free election in a communist country attracted a surprisingly low turnout. Less than two-thirds (63.6%) of those eligible to vote, which included all those over the age of 18, went to the polls. Turnout varied by district between a low of 53.2% in Ostroleckie to a high of 71.4% in Leszczynskie. By recent U.S. standards, such levels of turnout are not unusual; but they are low when compared to the multiparty democracies in Western Europe (Piven and Cloward 1988). A Polish plebiscite in 1989, however, offered an unprecedented opportunity to repudiate the ruling elite in favor of the popular opposition movement that this government had gone to such lengths to squash. It is difficult to know, considering the dearth of comparable elections, whether one should be surprised that a vigorous social movement could not pull more votes, or impressed by the electoral support a nascent political organization received in the teeth of state power.

Political scientists have generally found that voting at the individual level is a function of education, age, and socioeconomic status but that close, highly competitive elections promote partisanship and voter turnout (Jackman 1987; Kim, Petrocik, and Enokson 1975; Schaffer 1981; Wolfinger and Rosenstone 1980). Piven and Cloward (1988) argue that the low rates of turnout in U.S. elections reflect a demobilized electorate with weak partisan loyalties, the product of a party structure accustomed to diluting issues and compromising the class interests of the constituency.

Data on voter participation by voivodship indicate that the relationship between support for Solidarność and turnout in the Polish election was curvilinear; the larger the proportion of the voivodship that supported Solidarność or, conversely, supported the national list, the stronger the turnout. The lowest turnout was observed in the middling ranks of the distribution of support for Solidarność; the voivodships with both the highest and the lowest levels of support voted in the largest numbers. In only two voivodships did a majority of the voters support candidates who were not running for the Solidarność slate; in these two voivodships over 60% of the electorate supported the national list, and turnout averaged 70%.[6] In the five voivodships in which more than three-fourths of the votes went to Solidarność candidates, 62% of the voters rejected the national list outright, while turnout averaged 69%.

The patterns of turnout in the Polish election suggest that the more intense the level of partisanship within a voivodship (whether for or against Solidarność), the greater the likelihood of voting. Unlike patterns in the West, neither close races nor narrow victories distinguished the voivodships with the largest turnout. To be sure, the terms of this election assured that the national list would not face an open competition with Solidarność candidates; however, the degree of polarization between government and opposition is quite deep.

An alternative explanation would be that political organization, rather than intense support, stimulated turnout. In the voivodships best organized in support of either the Solidarność candidates or the government slate, voting would be expected to increase. Efforts to mobilize voters, however, were generally low-key throughout Poland. Solidarność volunteers offered detailed instructions and sample ballots near the voting places, while the government paid the election officials. In Warsaw, informal canvassing suggested that low turnout indicated skepticism and indecision, rather than apathy. There was a distinct fear that the accords of the round table would not be honored by General Jaruzelski and that martial law might be reinstated. Whether due to ambivalence, fear, or indecision, the voivodships that supported Solidarność the most and the least, or those in which the government had the strongest and the weakest support, experienced the highest turnout.

The curvilinear relationship between turnout and support for Solidarność resembles a parobolic function; by including a quadratic term in the equation predicting turnout from the support for Solidarność, a reasonably good fit is obtained. In equation 1, 65% of the variance in turnout (T_i) across voivodships is explained by the proportion supporting Solidarność, (S_i) and (S_i^2).

$$T_i = a + b_1S_i + b_2S_i^2 + e_i$$

$$T = 165.24 - 3.5054S + .0293S^2, \quad (1)$$

with t-scores of 12.3, -8.0, and 8.4, respectively and an adjusted R-squared of .61. Rearranging terms, equation 1 is equivalent to

$$T = (a - b_1^2/4b_2) + b_2(S + b_1/2b_2)^2,$$

where the constant term, $a - b_1^2/4b_2$, defines the minimum value of T for the function, while $(S + b_1/2b_2)^2$ defines the point on the x-axis where this minimum value occurs, at $S = -b_1/2b_2$.[7] By substitution, the quadratic equation implies a minimum value of 60.5% turnout in this election; at this minimum point, 59.8% of the voters are predicted to support Solidarność.[8]

Determinants of Social Support for Solidarność

Aggregate data provide a meager basis for testing the effects of specific social characteristics on the levels of support for Solidarność; ecological fallacies are an inherent risk if one wishes to predict individual votes. Unfortunately, individual-level voting data are not available; few preelection or postelection polls were authorized, and the data from those that were have not yet been released. The major conclusion from the aggregate election data, however, is that the strongest relative support for Solidarność in this election was concentrated in poor, rural areas and not in the traditional strongholds of the urban working class. The models that support this conclusion are quite robust, irrespective of the measures used. Across voivodships, Gdansk ranks twelfth in proportion supporting Solidarność, while the 10 most supportive areas include nine voivodships in the rural southeast. The aggregate variables related to voting behavior provide a way of describing and teasing out various interpretations of this phenomenon, although they do not enable us to predict the voting behavior of individuals.

There are three conventional rationales for analyzing aggregate data: (1) the units of analysis are inherently meaningful; (2) inferences based on individual behavior are unnecessary for an adequate explanation; and (3)—weakest of all—disaggregated data are not available. In this election, all three apply. The voivodships acquired their current boundaries in 1975; they are the chief administrative centers for implementing economic plans, distributing goods, and negotiating labor disputes. Solidarność established organizational centers that coincided with the boundaries of the voivodships in the early 1980s (Bivand 1983). Moreover, economic decentralization and representative forms of government are likely to

Table 1. Solidarnośc Supporters and National List Supporters by Region and Percentage Urban

Percentage Urban	Solidarnośc Supporters (%)	National List Supporters (%)	Number of Voivodships	Population over 18 (thousands)
North and West Poland	62.7	49.0	38	21,757
Less than 40% urban	61.4	52.3	4	1,278
40–50% urban	61.9	49.2	11	4,533
51–62% urban	58.6	55.2	11	4,559
Over 62% urban	64.9	46.1	12	11,385
Southeastern Poland	74.9	39.5	11	4,726
Less than 40% urban	74.7	39.8	7	2,431
40–50% urban	76.2	41.2	2	709
51–62% urban	72.7	41.1	1	702
Over 62% urban	76.5	32.2	1	884
Total	65.0	47.1	49	26,483

strengthen voivodships and reinforce the ecological foundations of political culture in Poland.

The restored Senat is dominated by Solidarnośc candidates; they are not, however, united by a common platform or by shared goals. In Poland (as in many other countries) the political geography of voter representation favors the rural regions over urban areas. Nearly half (46%) of the members of the Senat represent regions in which less than half of the voters live in urban areas; the total population in these voivodships is equal to roughly one-third of the country. The strongest and most consistent support for Solidarnośc was in these rural voivodships, particularly in the southeast. Table 1 presents the support for Solidarnośc and the national list by the percentage of the population living in urban areas in a voivodship and by region. Although percent urban has a small negative correlation with the proportion voting for Solidarnośc, the most rural areas with the smallest population per voivodship backed the Solidarnośc slate most solidly. The farm population, or the peasantry,[9] are far more concentrated than industrial workers. In six voivodships, the majority of voters are independent farmers, although peasants constitute less than one-fourth of the total Polish population; in contrast, the majority of males were industrial workers in only one voivodship. In 23 of the 49 voivodships, the estimated number of votes for Solidarnośc cast by independent farmers exceeded the number cast by workers, despite the fact that workers outnumber peasants in Poland as a whole.[10] Although most of the candidates for office were members of the intelligentsia, the number of seats in the Senat indebted to the votes of peasants suggests a significant farm constituency.

The purposes served by a descriptive analysis of aggregate voting patterns are twofold: to document the patterns of voting in rural and urban voivodships that demonstrate the shift of support from urban to rural regions, and to attempt an account of the patterns of voting within sectors. Two dependent variables are critical: the mean proportion of voters supporting Solidarnośc candidates in each voivodship and the proportion that voted for the national list. Although the two measures are not truly equivalent, (nor were the candidates actually competitive), the votes represent the degree of support for Solidarność and for the dominant party in Poland. The distribution of major voting blocks by voivodship and the sources of support within them depict major cleavages in Polish society and the sources of potential conflict within the Sejm and the Senat. Rural votes suggest a strong, independent source of power that has not been traditionally represented in Polish politics.

Although individual-level data on voting patterns are not available for this pivotal election, national survey data support the conclusions of the aggregate analysis. A year before the round table talks, in May 1987, the Institute of Philosophy and Sociology of the Polish Academy of Science conducted a survey of nearly two thousand Polish adults, as part of a larger study of social structure and mobility.[11] Several items tapped attitudes toward the present system of government, social change, and socialism in general.[12] Respondents were also asked whether or not they had belonged to Solidarność in 1980–81, when the union was first formed. Although actual membership in Solidarność in 1981 approached 10 million (approximately one out of every three Polish citizens), only 18% of the respondents sampled in 1987 reported that they had belonged.[13] At present, 20% of the population are dues-paying members of Solidarność. Although neither variable measures actual voting behavior, contrasting the individual-level determinants of political attitudes with the aggregated data supports the patterns discussed.

In order to compare the determinants of membership in Solidarność with attitudes toward the regime in general, two separate regressions were run, based on the individual-level demographic and socioeconomic information. Table 2 presents the equations predicting responses on

Table 2. Solidarność Memberships and Government Attitudes Regressed on Individual Characteristics, Poland 1987 (Standardized Regression Coefficients)

	Dependent Variables		
		Attitude Toward Government and Change	
Independent Variable	Membership in Solidarność in 1980–81	Without Solidarność Membership	With Solidarność Membership
Urban/rural	.114****	.055*	.046
Age	-.082***	.016	.022
Sex	.068**	.116****	.110****
Education	.080**	.107***	.100***
Party member	-.045*	-.105****	-.102****
Occupational status	-.003	.049*	.050*
Wages	.007	.041	.041
Solidarność member	—	—	.077***
Peasant/farm	-.081**	.118****	.124****
Industrial worker	.164****	.056	.043
R^2	.09	.04	.05
Number of cases	1,894	1,894	1,894

Note: Significance is based on one-tailed tests.

$^{*}p \leq .05$.
$^{**}p \leq .01$.
$^{***}p \leq .001$.
$^{****}p \leq .0001$.

these two items regressed on age, sex, years of education, occupational prestige, whether the person resided in a town or village, individual income, and membership in the Communist party. In addition, two dummy variables were created indicating whether the respondent was a peasant (20% of the sample) or a skilled worker (29%).

Membership in Solidarność in 1980–81 was, not surprisingly, most common among young males, skilled workers, those living in urban areas, the better-educated, and non-party members (or at least as of 1987).[14] Antigovernment attitudes, in contrast, are more evenly distributed by age and residence, although males and non-party members express more desire for change. Industrial workers were 16% more likely to be Solidarność members than were other respondents, while the gap in membership between peasants and nonpeasants was 14%. Among peasants, however, hostility toward the government and support for Solidarność does not depend on group membership;[15] less than 2% of the peasants reported belonging to Solidarność in 1981, in contrast to almost 30% of the workers.[16]

Antigovernment sentiments, in contrast, are far more prevalent among the peasantry than among industrial workers, whether or not self-reported membership in Solidarność is controlled. Workers are more opposed to the system of government than the average nonfarm citizen, but peasants are perhaps three times more strongly opposed. Even with other individual characteristics controlled, there are strong residual differences between the workers and the peasants. Polish peasants in 1987 were more hostile to the system of government and more supportive of change than were workers who claimed to have been members of Solidarność in 1981.

The aggregate variables relevant to this analysis can be divided into three clusters.

1. Electoral variables measuring differences in outcomes of the election (e.g., proportion voting for Solidarność, proportion rejecting the national list, turnout, and number of candidates on the ballot relative to available seats).
2. Urban/industrial variables describing differences among the voivodships in terms of relative urban or industrial composition (e.g., proportion of population living in urban areas, sale of newspapers per 1,000 population, per capita consumption of electricity, proportion of population with university degree, number of cars per person, population in 1986, mean state salary in 1984, hours of absence from work in 1984, and proportion of state employment in industry), calculated from official statistics published by the Polish government (Poland, Glowny Urzad Statystyczny 1988).
3. Rural/agriculture variables depicting differences among the rural voivodships (e.g., proportion of population working in private agriculture, farm construction per 10 farms, size of farms in hectares, percentage of population working in both agriculture and industry, percentage farm population, and dummy variable placing voivodship in the southeast or not).

These variables, with their means, standard deviations and zero-order correlations with electoral outcomes, are presented in Table 3.

As the data in Table 3 make clear, the independent variables with the strongest and most consistent relationship to aggregate electoral outcomes tend to distinguish the rural voivodships. The ecological patterning of Solidarność votes confounds the proportion urban with the variables measuring working class support; larger proportions of the variance in the characteristics associated with the rural areas are between voivodships, which are more homogeneous and much

Table 3. Means, Standard Deviations, and Correlations with Electoral Outcomes: Aggregate Analysis, Poland 1989

			Correlations of Electoral Outcomes	
Variables	Mean	Standard Deviation	Solidarność Supporters	National List Supporters
Electoral				
Solidarność (%)	64.96	7.75	1.00	−.69
National list (%)	47.08	7.56	−.69	1.00
Turnout (%)	62.66	3.53	.13	−.15
Candidates per seat	5.94	2.12	−.21	−.22
Urban/industrial				
Urban (%)	61.35	18.79	−.10	−.12
Sale of news/1,000 population	85.70	20.98	−.06	−.13
Electricity consumption	958.38	410.99	.18	−.28
Higher education (%)	9.63	3.27	.30	−.55
Cars/1,000 population	112.82	30.02	.06	−.21
Total population	1,248.84	1073.20	.09	−.16
Mean state salary 1984	16,545.43	2338.06	.06	−.06
State employees in industry (%)	35.01	8.97	.02	−.08
Rural/agricultural				
Working private agricultural (%)	13.27	10.32	.13	−.03
Construction/10 farms	10.20	3.30	−.10	.05
Hectares/tractor	17.32	3.46	−.42	.41
Farm and state employment (%)	5.08	4.34	.34	−.24
Hours work absence 1984	191.39	17.52	.46	−.52
Peasant (%)	19.69	17.28	.12	.05
Southeast	.18	.38	.62	−.54

smaller in size. As a consequence, multivariate models of support for Solidarność are more likely to include agricultural variables. In each of the models to be tested, the independent variables with the most robust relationships to voting outcomes tend to be those that distinguish the rural voivodships, particularly the 10 voivodships located in the southeast. Table 4 presents the regressions of the independent variables that contribute the largest explanatory power to the voting patterns on the proportion supporting Solidarność and the proportion not rejecting the national list. Each of the two dependent variables are also presented with the other variables controlled for. The correlation matrix is given in the Appendix, Table A-2.

These variables were the most likely to be significantly different from zero, irrespective of alternative controls. They were consistently related to both dependent variables, taken individually or in combination. Only one voivodship differs in degree of support for Solidarność from the value predicted by the model by more than three standard deviations; when this outlying voivodship is deleted, the parameters of the equation are virtually unchanged. Finally, the estimated effects are consistent; restricting the sample by excluding the five largest and most populous voivodships does not change the equations perceptibly.

The two variables included from the urban cluster, proportion of population with higher education and proportion of state employment in industry, diminish substantially when both dependent vari-

Table 4. Aggregate Outcomes Regressed on Characteristics of Voivodships, Poland 1989 (Standardized Regression Coefficients)

	Dependent Variables			
	Solidarność Supporters (%)		National List Supporters (%)	
Independent Variables	Without National List Variable	With National List Variable	Without Solidarność Variable	With Solidarność Variable
Workers in private agriculture (%)	.354*	.120	.584****	-.560***
Farm construction/farm	-.424****	-.320****	.432****	.354**
Hectares/tractor	-.512****	-.446****	.210**	.131
Farm and state employment	.270****	.309**	-.171	-.128
Employment in industry (%)	-.003	-.213**	-.469***	-.469***
Hours work absence 1984	.224****	.137*	-.164*	-.128
Higher education (%)	.371***	.001	-.908****	-.849****
National list	—	-.366****	—	—
Solidarność	—	—	—	-.158*
Adjusted R^2	.58	.60	.77	.78

Note: These equations give the estimated coefficients predicting an electoral outcome with and without the alternative measure held constant. One-tailed tests have been used.

$^{*}p \leq .05$.
$^{**}p \leq .01$.
$^{***}p \leq .001$.
$^{****}p \leq .0001$.

ables are included simultaneously. The effects of higher education, for example, appear to be much stronger in predicting votes against the national list than in predicting support for the Solidarność candidates.

The equations presented in Table 4 provide the most consistent estimates of the determinants of support for Solidarność among voivodships. The variables that best discriminate levels of support among voivodships are also the variables that differentiate voivodships by characteristics of the farm population. In part, the small effect of the variables that measure worker strength is due to the distribution of workers throughout the country. Peasants are simply more concentrated geographically. Aggregation biases are more likely to confound the estimation of effects when the proportion of variance between units is low (Hannan and Burstein 1974; Langbein and Lichtman 1978). Although it is not possible to estimate the degree of aggregation bias without individual-level data on the dependent variables of interest, Table 5 presents the zero-order correlations and the proportion of variance between voivodships based on the variables analyzed from the 1987 survey data. As the table clearly indicates, grouping by voivodship restricts the variance between voivodships far more for the variables measuring characteristics of workers than for the variables measuring differences in urban location or peasant origins. Inferring that the characteristics predicting support for Solidarność among individuals are comparable to those estimated by voivodships is in-

Table 5. Estimated Aggregation Bias Introduced by Using Voivodships As Units of Analysis

Independent Variables	Zero-Order Correlations: Government Attitude	Zero-Order Correlations: Mean Government Attitude	Variance between Voivodships (%)
Urban	.02	.13**	15.82
Age	.03	-.02	2.41
Sex	.11**	-.01	1.40
Education	.05	.11**	7.64
Party	-.08**	.02	2.15
Occupational status	.07*	.07*	5.64
Wages	.07*	.06	5.25
Peasant	.03*	-.06	15.78
Worker	.03	-.02	5.27
Solidarność	.09**	.05	6.32

Note: One-tailed tests have been used.

$^*p \leq .05$.

$^{**}p \leq .01$.

herently hazardous, but perhaps especially so for the variables distinguishing characteristics of industrial workers.

Although one cannot infer individual differences from the models describing Solidarność support, the models suggest a plausible descriptive summary of the determinants of aggregate support among the rural voivodships. The single variable most significantly and consistently related to aggregate voting behavior is location in one of the rural southeastern voivodships.[17] These voivodships are characterized by a large population of peasants with small, privately owned farms. Less is spent on farm construction and fewer people engage in full-time state employment. In Poland as a whole, only 5% of the population works in both agriculture and industry; in the southeast the percentage is 12%. Prolonged absence from work is common, as one would expect when many factory workers have agricultural responsibilities as well. These are precisely the characteristics that distinguish the voivodships supporting Solidarność.

Polish sociologists provided a number of possible explanations for the strength of support for Solidarność in the southeast, hypotheses that unfortunately cannot be readily tested with existing data (Florczyk, Zukowski, and Najdowski 1989; Kaluza 1989; Raciborski 1989). The southeastern region has experienced substantial emigration but little immigration since World War II. Collective farms are virtually nonexistent. The population is reputed to be among the most traditional and the most committed to Catholicism in the country (Hruby 1982/83; Korbonski 1984). In the countryside, it is difficult to exaggerate the importance of the local parish for communication and mobilization. Under the leadership of Bishop Tokarczuk, who has the reputation of being very sympathetic to opposition organizations, the church was crucial in facilitating the organization of civic committees in this region.

The bulk of informal economic activity in Poland is concentrated in the agricultural sector, and the south does more than a fair share. The average size of farms is small, half as large as in the country as a whole. Although quite poor, the households in this region are alleged to receive hard currency (and perhaps Western political attitudes as well) from relatives abroad. Analysts disagree about the political significance of the shadow economy, in particular regarding whether or not it generates attitudes and behavior antagonistic to the state (Sampson 1988); it is clear, however, that Polish peasants are relatively dependent on the informal economy, whether or not such activity fosters liberal ideologies.

In Poland, the vast majority of self-employed persons are independent farmers; this may be the critical factor explaining peasant support for Solidarność. Political opposition in the southeast is longstanding; these voivodships were the most likely to abstain from voting or to strike out the names of state candidates in the elections held in the 1950s (Wiatr 1962). Historically, the southern rim of Poland was dominated by Austria and the Hapsburg Empire; one informant suggested that democratic ideas and relatively democratic institutions were more common in this region in the nineteenth century than in other parts of the country.

In short, there are a wealth of explanations for the ecological patterning of support for Solidarność. Historical, political, and economic factors may all be involved. Both rural and urban support for Solidarność are rooted in antigovernment sentiments; peasant support will, however, be a critical factor in the future. Whether the politics of the peasantry has evolved from the connection to a free market (Nee 1989; Nee and Stark 1989); the democratic, nationalist, or religious ideals of the populace; or geographic and historical happenstance unique to Poland—it remains a factor that future governments will have to take into account. In particular, economic reforms that advantage industrial sectors or obstruct the market activities of the peasantry are likely to be sources of conflict for the new government.

Conclusions

Apart from the statistical analysis of voting behavior, Polish electoral data provide evidence pertinent to questions about the characteristics and the very nature of Solidarność that we posed at the beginning. Two points should be made in this respect. The strong relationship between support for civic committee candidates and rejection of the national list clearly indicates that antiregime feelings are virtually inseparable from pro-Solidarność attitudes in Poland. Whatever diverse tendencies were present, the dismantlement of the communist state was the principal unifying objective. Solidarność retains certain characteristics of a trade union, but this organizational legacy appears to have been a function of the forms of political expression initially tolerated by the state. The process of negotiating for, and then winning, this first electoral campaign transformed the social movement into a political organization with a broad base of support.[18]

The second point concerns the future of Solidarność as a political movement, particularly as the early consensus formed by opposition to the regime fades. The critical questions shaping the future of political life in Poland concern whether Solidarność can retain the broad-based coalition built on hostility to the communist regime; whether it will split and if so, when; and what other political interests or alternatives will emerge. At the moment, neither political alternatives nor economic interests are well articulated. The only strata that seems capable of building a political identity or party structure based on economic interests is the peasantry. Ironically, despite the strong relative support for Solidarność in rural areas, peasant organizations initiated the first collective opposition to the Mazowiecki government.

We have argued that the revival of Solidarność, combined with the acceptance (albeit reluctantly) of power, places the social movement squarely in the tradition of electoral politics and political reform. The era of the "self-limiting" revolution, seeking democratization rather than political power, had ended. Whether the Solidarność representatives and the government they have formed can retain mass support and the endorsement of the diverse constituencies that elected them remains to be seen.

Appendix

Table A-1. Characteristics of Polish Voivodships, 1989

Voivodship	Solidarność Voters (%)	National List Supporters	Urban	Working Private Agriculture	Total Population 1989 (thousands)
Bialostockie	59.3	51.6	60.0	22.51	679.9
Bialskopodlaskie	72.2	45.1	34.4	34.47	301.1
Bielskie	70.2	45.7	49.0	8.76	884.7
Bydgoskie	51.6	52.8	63.6	9.20	1,096.6
Chelmskie	55.7	58.4	41.3	27.19	243.5
Ciechanowskie	61.5	56.6	34.8	29.38	422.2
Czestochowskie	68.1	46.9	52.0	13.99	771.3
Elblaskie	57.9	57.4	59.8	10.24	472.2
Gdanskie	70.6	44.5	76.0	5.46	1,419.8
Gorzowskie	56.4	57.1	61.2	7.56	490.6
Jeleniogorskie	62.7	43.4	66.1	5.24	513.9
Kaliskie	64.5	55.4	45.5	18.22	703.7
Katowickie	65.2	48.4	87.7	2.09	3,970.8
Kieleckie	65.3	46.9	45.6	21.91	1,115.6
Koninskie	58.1	47.3	39.9	23.92	463.4
Koszalinskie	56.0	60.8	61.7	6.85	498.4
Krakowskie	76.5	32.2	69.3	8.86	1,216.5
Krosnienskie	74.6	39.6	34.0	17.83	483.7
Legnickie	67.8	42.4	68.2	8.31	502.7
Leszczynskie	43.6	62.3	47.0	14.84	380.4
Lodzkie	53.7	44.2	91.5	2.73	1,148.4
Lomzynskie	72.0	44.0	38.1	40.44	342.2
Lubelskie	72.7	41.1	56.9	22.27	997.3
Nowosadeckie	77.8	40.8	36.0	24.87	679.4
Olsztynskie	55.9	55.9	58.3	11.46	738.6
Opolskie	65.2	51.1	51.5	10.58	1,022.7
Ostroleckie	65.0	52.1	32.5	35.05	389.5
Pilskie	38.3	62.3	54.9	10.35	472.6
Piotrkowskie	61.3	45.9	48.1	20.30	639.2
Plockie	66.0	48.4	46.9	25.07	512.8
Posnanskie	67.6	55.8	70.5	6.14	1,316.1
Przemyskie	75.2	44.5	36.9	25.65	400.5
Radomskie	54.6	43.6	46.2	27.30	736.5
Rzeszowskie	80.3	38.3	40.2	20.07	703.7
Siedleckie	62.1	49.9	30.7	35.80	642.0
Sieradzkie	57.0	51.9	35.3	28.76	403.7
Skierniewickie	67.3	50.2	45.3	23.65	413.2
Slupskie	54.7	61.6	54.6	8.07	403.9
Suwalskie	62.9	54.4	51.9	19.58	458.9
Szczecinskie	60.7	52.0	74.6	4.69	958.6
Tarnobrzeskie	71.6	38.7	36.3	28.32	587.0
Tarnowskie	74.8	33.8	35.2	22.95	651.8
Torunskie	67.0	55.0	62.2	13.72	649.6
Walbrzyskie	69.7	49.5	73.3	4.88	739.2
WARSZAWA	67.1	36.6	88.5	2.91	2,431.8
Wloclawskie	58.0	52.0	46.2	25.02	428.1
Wroclawskie	71.7	36.4	73.5	6.13	1,121.1
Zamojskie	76.4	36.2	26.9	40.55	489.9
Zielonogorskie	56.2	59.7	59.9	6.13	654.9

Table A-2. Means, Standard Deviations, and Correlations Based on Voivodships, Weighted by Voting Population

	Variables								
Variables	Solidarność Supporters (%)	National List Supporters (%)	Private Agricultural Workers (%)	Farm Construction/ Farm	Hectares/ Tractor	Farm and State Employment	Employment in Industry (%)	Hours Work Absence 1984	Higher Education (%)
Solidarność supporters (%)	—	-.69	.13	-.10	-.42	.34	.02	.46	.30
National list supporters (%)	-.69	—	-.03	.05	.41	-.24	-.08	-.52	.55
Private agriculture workers (%)	.13	-.03	—	.32	.18	.85	-.31	-.30	-.37
Farm construction/farm	-.10	.45	.32	—	-.41	.27	.15	.05	-.01
Hectares/tractor	-.42	.41	.18	-.41	—	.00	-.49	-.53	-.18
Employment in industry (%)	.34	-.24	.85	.30	.00	—	-.08	.09	-.32
Hours work absence 1984	.46	-.52	-.30	.05	-.53	.09	.45	—	.22
Higher education (%)	.30	-.55	-.37	-.01	-.18	-.32	-.35	.22	—

Note: N = 49.

Notes

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1. Jacek Kuron and Adam Michnik, two of the more radical leaders of the opposition, are cases in point.

2. Stefan Nowak, personal communication.

3. The two candidates who were endorsed by a slim majority of the electorate included a popular celebrity who had participated extensively in reform efforts and a relatively unknown figure who was listed last on the ballot, alphabetically. Knowledgeable observers speculated that this last victorious candidate was elected in part as a result of voter carelessness, rather than voter support. Most voters rejected the national list by drawing a large 'X' through the entire ballot sheet; the election rules specified, however, that if a voter's pencil had not touched the name of a candidate, he or she would be considered elected; apparently, a significant number of voters neglected to mark through the complete list.

4. Correlations weighted by the number of voters tend to be somewhat lower than those that weight each voivodship equally, because the smaller voivodships are more homogeneous and more rural. Weighted correlations deflate the influence of these voivodships relative to the larger and more urban centers. The relationship between the political outcomes and the composition of the voivodships are also slightly stronger. The weighted correlation between support for Solidarność and support for the national list was $-.69$, for example.

5. In standardized form, $T = -.776N - .281C$.

6. The two voivodships are Pilskie and Leszczynskie, both located in the northwestern region of contemporary Poland that was reclaimed from Germany after the war. Both have large numbers of resettled Polish citizens, who owe both their land and their allegiance to the communist government.

7. If b_2 is negative, the value of S is equal to the maximum for the function.

8. A polynomial function with two inflection points fits the data somewhat better than the quadratic, particularly at the extremes. The higher-order terms are, as expected, highly correlated with support for Solidarność. By setting the acceptable tolerance level to .001, however, a polynomial regression line can be fit, despite multicollinearity among the independent variables. Adding the cubic term, S^3, increases the percentage of explained variance to 98% and yields the following equation:

$$T = 76.06 - .8292S + .0026S^2 + .00011S^3$$

9. *Peasant* is a widely used term in Poland with a specific class connotation and no equivalent in the United States. We use the term interchangeably with *independent farmer.*

10. Estimates of the relative voting strength of peasants and workers are based on ecological regressions that assume the probability of supporting *Solidarność* is constant for each group across voivodships (Goodman 1959; Langbein and Lichtman 1978).

11. The data from the 1987 survey were collected by the research team at the Institute for Philosophy and Sociology at the Polish Academy of Science, which includes Ireneusz Bialecki, Henryk Domanski Krystyna Janicka, Bogdan Mach, Zbigniew Sawinski, Johanna Sikorska, Kazimierz M. Slomczynski (principal investigator), and Wojciech Zaborowski. The sample (N = 1,894) consists of a random sample of the economically active civilian population, men aged 18–65 and women aged 18–60.

12. The question assessing support for the government consisted of responses to a four-point scale about the current system of political power in Poland: the system was to be scored as (1) good, with no change needed; (2) good but needing some change; (3) having shortcomings, needing change but with stability; and (4) bad and needing fundamental change. A second question asked whether the socialism had brought for most people in Poland (1) advantages only, (2) more advantages than losses, (3) equal advantages and losses, (4) more losses than advantages, or (5) losses only? This item correlates reasonably well with the previous item, but with far more nonresponse.

13. Solidarność membership was concentrated, as one might expect, in the cohort whose members were in their twenties in 1980.

14. The 1980–81 Polish *odnowa* (renewal) that gave birth to Solidarność included large numbers of party members. Ash (1983) estimated that one-third of all party members (close to a million people) belonged to both Solidarność and the Communist party in 1981. The imposition of martial law led to widespread defection, however; and membership declined. Reform movements sweep up the young, the educated, and the most politically active—groups that include quite a few party members. While it may no longer be possible to mobilize political reform through the party in Poland, reform movements within the Communist party have become the norm in Central Europe.

15. Rural Solidarność was never as large or as active as the industrial unions, in part because three separate organizations competed for leadership of the peasants' union (Halamska 1988).

16. The percentage differences cited in the text differ from those in the table because they are based on the coefficients in equations that include workers and peasants separately. The coefficients in Table 2 are deviations from the omitted category, which in this case is the group who were neither peasants nor workers in their primary job.

17. Chelmskie and Bialostockie, the two rural voivodships in the southeastern region that gave the least support to Solidarność differ in a number of crucial respects. The population is considerably older and more dispersed than is the case in the rest of the region. They are less likely to be involved in nonfarming activities, and their lands tend to be less productive. Moreover, the peasants in these regions originally leased, rather than owned, their land, receiving title only after the agricultural reforms in the late 1940s.

18. The local elections in May 1990 underscored the effectiveness of the network of civic committees as political organizations.

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A GAME THEORETIC MODEL OF REFORM IN LATIN AMERICAN DEMOCRACIES

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In this article I develop a simple game-theoretic model of administrative reform in Latin American democracies. The model, which is based on the incentives facing the politicians who must initiate reforms if any are to occur, yields two predictions: (1) reforms are more likely to pass the legislative hurdle when patronage is evenly distributed among the strongest parties, and (2) initial reforms are more likely to be followed by further extensions of reform where the electoral weight of the top parties remains relatively even and stable. Attention to the incentives facing legislators and party leaders also results in the expectation that certain political institutions, such as open list proportional representation and electoral rules that minimize party control over candidate lists, reduce the probability of reform. I test these predictions and expectations on a set of Latin American democracies and find them consistent with historical events.

Bureaucratic inefficiency, patronage-induced overstaffing, and outright corruption retard economic development and reduce public well-being in developing countries. They prevent governments from effectively carrying out the economic plans to which they devote so much official attention, and deprive citizens of government services to which they are legally entitled.

The costs associated with bureaucratic deficiencies are widely recognized. Nevertheless, the initiation of reforms has proved difficult. In this essay I explain why reforms that are widely regarded as necessary and desirable often face such severe obstacles to their initiation. The explanation lies in the interests of the politicians who must make the decisions that would promote or impede reform. The heart of the argument is that these individuals frequently face a choice between actions that serve their individual political interests and actions that would improve the long-run welfare of their societies and that when this happens, individual interests generally prevail. Reforms only occur in political circumstances that render the individual interests of the politicians who must initiate them consistent with the collective interest in reform.

Struggles over administrative reform have developed in virtually all countries in which political competition preceded the establishment of a professionalized state apparatus. The initiation of reform entailed as bitter a struggle in Britain and the United States during the nineteenth century as in Latin America during the twentieth century. Numerous case studies of the initiation of reforms exist—but few general explanations.

I suggest a general explanation. In this article I shall use simple game-theoretic models to explore how different electoral and party systems affect the incentives of legislators to initiate reforms. The analysis focuses on one type of reform, the introduction of merit-based hiring for civil servants. This aspect of reform was selected for emphasis because the many administrative reform packages proposed during recent decades nearly always include it; because rules for merit-based hiring, unlike other kinds of reform, vary only moderately from country to country; and because the effects of laws requiring recruitment by exam are relatively easy to assess. Meritocratic recruitment is not always the most important aspect of administrative reform; but it is always at least moderately important, and it is the easiest element of reform to "measure" and compare across nations.

I analyze the legislative struggle over reform in South American democracies,[1] of which some have initiated reforms, and some have not. The choice of this set of cases holds roughly constant several variables that are often mentioned as possible causes of honesty and competence in government: culture, colonial institutional structure, and level of economic development. At the same time, it preserves sufficient variance in contemporary political institutions and reform outcomes to allow the testing of hypotheses.

The Need for Reform

Demand for reform in Latin America resulted from widespread recognition that the traditional use of government resources for partisan purposes had led to excesses. In Uruguay during the 1950s and 1960s, for example, so large a fraction of the budgets of many government agencies went for wages to pay patronage appointees that there were no funds left for operating expenses. In 1960, Montevideo newspapers reported that because the Ministry of Public Health had created 1,449 new jobs in public hospitals, no money was left to buy medicines and essential hospital equipment (Taylor 1960, 222). The Ministry of Public Works hired 313 new employees during one seven-month period to repair equipment. Since no funds remained to buy spare parts, by the end of the period none of the department's many self-propelled road scrapers remained operable, and all work on the roads had ceased. The "entire budget was being used to pay personnel and none was left for fuel, equipment, and materials" (Taylor 1960, 103, 178–79).

Overstaffing is only one of the forms government inefficiency can take. In 1983 a group of Peruvian researchers set out to acquire a license to operate a small workshop in Lima. Documenting every step and moving as expeditiously as procedure allowed, they found that it took 289 days to obtain the 11 separate licenses and certificates legally required. Along the way, they were solicited for bribes 10 times (de Soto 1986, 173–75).

As governments in the region began trying to promote economic development and as state intervention in economies increased, the need for administrative reform to increase the effectiveness of developmental strategies became increasingly obvious and urgent. Technical experts claimed that intervention in the economy would not produce desired effects unless the individuals making and carrying out policies were better trained (e.g., Emmerich 1960, 1972; International Bank for Reconstruction and Development 1961), presidents regularly proposed administrative reforms as elements of their development strategies (e.g., Brewer-Carías 1975a; Morcillo 1975), political party leaders espoused support for reform (Brewer-Carías 1975a; Groves 1974; Urzúa and García 1971), the press campaigned energetically against corruption and incompetence in the bureaucracy and called for reform (González 1980; López Pintor 1972; Taylor 1960; Urzúa and García 1971), and ordinary people expressed a desire for more competent and honest government in their answers to survey questions.[2]

Impediments to Reform

In spite of widespread support, however, reforms have occurred only slowly and sporadically. Two groups have opposed reform: those who have found in bureaucratic jobs a "refuge from which to make a last-ditch stand for their right to a quiet, incompetent existence" (Hirschman 1958, 154) and elected politicians and party activists. The opposition of employees who gained their jobs through patronage reflects the expected costs of reform to them. But perhaps less obviously, administrative reform is also costly to the politicians who must enact it. Traditionally, jobs in the bureaucracy and the multitude of contracts, subsidies, exceptions, and other scarce values distributed by bureaucrats have served as important electoral resources. Politicians and officials have been able to trade help in acquiring these resources for support (Biles 1972, 1978; Singer 1965; Valenzuela 1977).

Administrative reform threatens to eliminate these political resources. Reforms that introduce merit as the main criterion for hiring and promotion decrease the ability of politicians and party leaders to reward supporters with jobs. Efficiency criteria applied to bureaucratic procurement procedures reduce political discretion in the awarding of contracts. The use of economic and technical criteria rather than partisan favoritism to decide who gets subsidized credit, access to foreign exchange, tax exemptions, and so on further reduces the resources politicians can exchange for support.

The cost of administrative reform to politicians and party activists is thus clear. Under certain circumstances, however, administrative reform may also provide them with benefits. If the national economy improves as a result of increasing the competence of officials and the effectiveness of their decisions, incumbent politicians and their party can claim credit for it in the next election. Moreover, politicians and their party may gain support from voters who favor reform. Politicians may gain a sense of satisfaction from having helped provide the country with more honest and competent administration. A final possibility is that as the electorate grows, politicians and party leaders will prefer to switch from offering private goods in exchange for support to offering public goods, because, in a mass electorate, public goods cost politicians less per voter reached (Cox 1986).

To explain why reforms have occurred at some times and places but not others, then, one must answer the question, Under what circumstances will the benefits of reform outweigh the costs to the politicians who must at least acquiesce in passing them?

The Interests of Legislators and Party Leaders

Whether politicians will initiate administrative reform depends on the incentives they confront. Only if the individual aspirations for power, status, wealth, or policy change on the part of political activists and politicians can be furthered by the provision of reforms will they be provided. A formalization of the incentive structures faced by South American politicians making a choice about whether to use their limited resources to consolidate political support in customary ways or to bring about more efficient administration will yield predictions of when such reforms should occur.

Two groups of political actors play a role in the struggle over reform: elected politicians and party leaders (who often hold no elected office). Simplifying somewhat, one can hypothesize that these individuals want, above all, to further their political careers. Given this first-order preference, their strategic second-order preferences will vary, depending on the specific possibilities available to them.

The interests of elected officials in Latin America resemble the interests of politicians elsewhere. They want to be reelected,[3] and they prefer some policies over others. Without doing them too much of an injustice, we can assume that for most politicians most of the time, the desire to be elected takes precedence over policy preferences (see Ames 1987). For some, the desire for office and its perquisites truly overwhelms their commitment to particular policies. Others may want to be elected only in order to enact preferred policies; but if they fail to be elected, they lose their chance of influencing policy outcomes. Thus, even for the public-spirited, the preference for election will be strong, since election grants the opportunity to achieve other preferences (see Mayhew 1975). This is not to deny that for some politicians, ideological commitments outweigh the desire to be elected. The electoral process, however, tends to weed out such individuals; they are elected less frequently than those who consider winning of paramount importance. This selection process also contributes to the predominance of electoral motives among those who have already achieved office. As an initial simplification, then, suppose politicians compete with each other in a zero-sum electoral game, and that for those who compete in it, the desire to win this electoral game overrides other goals.

Party leaders further their careers by increasing the electoral success of their parties and by achieving greater influence within their parties. Many of their goals will thus coincide with those of politicians in their parties, since both politicians and party leaders benefit from policies that give their party electoral advantages. In certain other respects, however, their interests diverge. Party leaders benefit from policies that advance the interests of the party as a collectivity over the long run, whereas, in some situations, individual candidates may be injured by such policies. Party leaders, for example, favor rules that enforce party discipline; but in some circumstances individual legislators may be able to improve their electoral chances by breaking ranks. Situations may thus arise in which party leaders have interests not shared by the average legislator. Party leaders must then deploy the incentives at their disposal to influence legislators' votes.

The importance of party leaders in the struggle over reform depends on how much they influence candidates' electoral chances. Virtually all Latin American countries elect legislators using systems of proportional representation. The factors that contribute to the probability that candidates will win elections depend on how candidate lists are selected and whether lists are closed (placement on lists being determined by party leaders) or open (determined by voters). In open list systems, the vote for candidates depends on the popularity of their positions on issues, voters' party loyalty, candidates' personal charisma, and constituency services, patronage, and favors—much as it does in winner-take-all systems such as the United States. In closed list systems, the candidate's probability of winning depends primarily on the candidate's position on the party list. Factors such as charisma, issue position, and constituency service affect the candidate's probability of winning by influencing party leaders' decisions about placement on the list more than by influencing the vote directly.

In systems in which party leaders' control over the list determines who is elected, they exercise great power over the votes of politicians who have to be concerned about future reelection. In systems such as Brazil's (in which party leaders have virtually no control over who runs

Figure 1. The Effect of Patronage on the Probability of Election

		Politician 2	
		Merit	Patronage
Politician 1	Merit	v_1, v_2	$v_1 - x_2, v_2 + x_2$
	Patronage	$v_1 + x_1, v_2 - x_1$	$v_1 + x_1 - x_2, v_2 + x_2 - x_1$

Note: Politicians' utilities are assumed linear with the probability of winning the next election. V_i = the probability that candidate *i* will be elected if no jobs are distributed for political purposes. V_i rises and falls exogenously with normal political tides. $\Sigma v_i = 1$. X_i = the amount by which candidate *i* can increase his chances of election (at his opponent's expense) by rewarding supporters with government jobs. (It is assumed here that on average, each individual who works to maintain the candidate's political machine, get out the vote, etc., in the expectation of receiving a job will increase the candidate's probability of election by some positive, though possibly quite small, amount.)

for office) or Colombia's during the National Front (in which party leaders lost control of the lists) they exercise little power. Where party leaders determine election chances, the calculations of legislators considering a vote for reform will focus on the vote's expected effect on the judgments of party leaders.

Meritocratic hiring rules and other reforms that introduce impersonal criteria for the allocation of government resources reduce politicians' and party leaders' discretion over the distribution of the jobs and favors that fuel political machines and thus influence the vote. No politician in competition with others engaged in trading jobs and favors for votes can afford unilaterally to eschew reliance on patronage. Some, however, might be willing to give up this resource if others did. Similarly, no party leader could afford for the party unilaterally to cease distributing patronage; but some might be willing if they could be assured that others also would do so.

The conditions under which legislators can be expected to favor reforms that deprive everyone of some valued electoral resources can be deduced from a simple game-theoretic model, as shown in Figure 1. In this simplified world, each politician has some baseline probability of winning the next election, v_i, determined by voters' party loyalty, issue preferences, and so on. This baseline probability can be increased by the skillful use of political favors and decreased by his opponents' distribution of favors.

The payoffs in these matrices reflect the amounts of patronage to which a national party machine has access as a result of its participation in government. Because of the centralization of decision making and resources in Latin America, the patronage resources available to legislators depend more on their party's national position than on local resources.

The matrix in Figure 1 shows the incentives faced by candidates from different parties as they decide whether to use patronage to secure votes during an electoral campaign. The top left cell of the matrix shows the candidates' probabilities of winning when neither distributes favors; these are just the baseline probabilities v_1 and v_2. In the lower left cell, Politician 1 uses patronage, gaining an electoral advantage of x_1 from it; the opponent, who does not use patronage, then suffers a decline in probability of winning of $-x_1$. The upper right cell shows the reverse situation. The lower right cell illustrates the prereform milieu in which both candidates rely on patronage.

If this pattern of incentives were the

whole story, there would never be any reason for legislators to approve administrative reform. Each individual who relies on patronage is better off, no matter what others do and whether the game is repeated or not. If for any reason, however, legislators place a positive value on reform, no matter how small, the outcome of the game depends on the magnitude of the difference between x_1 and x_2 in relation to the expected political value of the reform. That is, if legislators value reform at all—if, for example, they think supporting reform would sway the votes of a small number of middle-class idealists or improve their standing by a tiny increment with party leaders—they might in some circumstances vote for reform.

Where popular demand for reform exists, giving legislators a reason to vote for it even though that reason rarely outweighs reasons to vote against, the incentives facing legislators as they decide how to vote are schematized in Figure 2. The lower right cell is the same as in Figure 1. Members of both parties vote against reform, so that neither can claim credit and neither is hurt by the other's claiming credit. The reform fails since neither large party voted for it and members of both parties continue to rely on patronage during election campaigns. The upper left cell shows the situation when both parties vote for reform. Since both voted for it, the electoral advantage of voting for it cancels out. The reform passes and neither party can rely on patronage during future campaigns. The upper right cell shows the payoffs that would result if the majority party voted in favor of reform and the minority party voted against. The reform passes so neither party can use patronage in future campaigns; and the majority party reaps a small electoral advantage, *e*, at the expense of the minority party from voting for reform. The lower left cell shows what happens when the majority party votes against reform and the minority votes in favor. The reform fails to pass so both parties continue to distribute patronage. The minority party gains a small amount of credit at the majority's expense for its vote for reform.

Whether legislators will vote for reform when they face the incentives schematized in Figure 2 depends on the relative magnitudes of x_1 and x_2. If $(x_1 - x_2) > e$, that is, if x_1 is any significant amount larger than x_2, the majority party will vote against reform, and it will fail. If, however, x_1 and x_2, are approximately equal, that is, if the two parties have approximately equal access to patronage resources, the majority party will vote for reform, and it will pass.[4] The minority party, as long as *e* has any positive value at all, will always prefer reform.

To reiterate in nontechnical language, in an election, all candidates can be expected to rely on patronage, as shown in Figure 1. Members of a party disadvantaged by the distribution of patronage resources, however, would be better off if

Figure 2. The Effect of Voting for Reform on the Probability of Election

		Legislator from Minority Party	
		Reform	Patronage
Legislator from Majority Party	Reform	v_1, v_2	$v_1 + e, v_2 - e$
	Patronage	$v_1 + x_1 - x_2 - e,$ $v_2 + x_2 - x_1 + e$	$v_1 + x_1 - x_2,$ $v_2 + x_2 - x_1$

Note: x_i and v_i are defined as in Figure 1. e = the amount of credit a legislator can claim for voting for reform.

the merit system were imposed on everyone. Thus, they always have an incentive to support reform in the legislature. Giving up the use of patronage would make them better off as long as everyone else also gave it up. As many observers have noted, the reform issue generally appeals to the "outs" in politics.

Where patronage is equally distributed and politicians can gain even a small amount from a vote for reform, members of both parties have reason to vote for it. In this situation, patronage conveys no relative advantage, but voting for reform may improve electoral chances. Consequently, political interest dictates the passage of reform. The matrix demonstrates the logic underlying these conclusions for political systems dominated by two large parties or coalitions, but the same argument would hold if three or more parties played central roles in electoral competition. Then patronage would need to be distributed approximately equally among all the top parties before it would be rational for legislators in the larger parties to vote for it.

Members of a party with greater access to patronage have no incentive to vote for reform in the legislature and no reason to eschew patronage during the electoral campaign either. They can improve their chances of being elected by relying on patronage, no matter what members of the other party do and no matter how long the situation continues. Members of a party with more access to patronage, then, could be expected to opt for reform *only* if they thought other gains from voting for reform would outweigh the certain costs that the loss of patronage would entail. One can imagine a situation in which public outrage over bureaucratic incompetence and graft had become so vehement that politicians might fear that they would lose more votes by voting against reform than by reducing their ability to distribute patronage. If public outrage very often reached such proportions, however, legislators would be eager rather than reluctant reformers.

Descriptions of the reform movement in the United States suggest that public outrage over the assassination of President Garfield by a disappointed office seeker played a catalytic role in bringing about reform (Van Riper 1958, 88–94). Even in this case, however, the legislature voted to pass the reform only after the election of 1882. This election, more than a year after Garfield's death, gave Democrats a majority in the House and thus marked the end of the Republicans' post–Civil War dominance over patronage. A few months prior to the 1882 election, the House had "derisively and angrily refused to give a paltry sum and to aid a single experiment of reform" (Hall 1884, 462). Immediately after the election, faced with the prospect of sharing the spoils in the immediate future, the same representatives who had ignored public opinion five months earlier passed the Pendleton Act by an overwhelming majority (Van Riper 1958, 94). Most of the time, legislators and party leaders can afford to ignore the public's desire for reform because reform is only one of many issues that affect voters' choices and is rarely—perhaps never—the most important issue to most voters.

To summarize the argument so far, the payoff matrices imply a prediction that spoils will be outlawed in democracies when two conditions are met, namely, that the benefits of patronage are approximately evenly distributed among the larger parties, and legislators have some small incentive to vote for reform.

Such reforms may turn out to be fragile if access to patronage again becomes one-sided, a subject to which I will return below. For now, let us look in more detail at how the game-theoretic prediction fares when confronted with evidence from the Latin American democracies.

The Effect of the Distribution of Patronage on Reform

In this section I shall test the predictions derived from the game-theoretic model on the universe of Latin American countries that have experienced 15 or more years of consecutive competitive democracy since 1930: Brazil, Chile, Colombia, Uruguay, and Venezuela. Costa Rica was excluded because its legislators cannot be immediately reelected, so that the simplifying assumptions about the interests of legislators used here could not be expected to apply to them. The 1930 cutoff date was chosen because interest in state-fostered development, and hence in administrative reform, began in most countries during the Depression. Prior to that time, administrative reform was not typically seen as an issue having much importance for public welfare.

Of the five countries examined, three—Colombia, Venezuela, and Uruguay—passed initial civil service reforms during more-or-less democratic periods.[5] Brazil's initial reform occurred during the Vargas dictatorship in the late 1930s. Most attempts to extend the reform during the democratic period failed. No comprehensive civil service reform passed in Chile during its long period of democracy. Below I examine the circumstances under which reforms occurred in Uruguay, Venezuela, and Colombia to see whether they are consistent with the predictions of the model. Then I discuss Brazil and Chile as contrasting cases.

In the real world there is no way to measure amounts of patronage or how much influence on the vote it has. In the case studies that follow, I deal with these measurement problems by relying on two plausible assumptions. First, all else equal, the distribution of jobs and favors increases the probability of being elected (otherwise politicians would not expend so much of their energy providing such services); the more a politician distributes, the better the politician's chances. Second, the amount of patronage to which a candidate has access for distribution depends on the party's control of elective and administrative offices at the national level, which in turn depends on the party's present and past electoral success. Electoral victories allow the more successful party to appoint its adherents to bureaucratic posts. These appointees become the source of the favors that influence later elections. (Generally, a waning party that has lost many of the seats it formerly held in the legislature will still be able to call upon loyalists in the bureaucracy for a number of years.)

These assumptions permit an approximate rank ordering of each party's access to patronage, a level of measurement sufficient for the simple model used here. They imply that if two parties control roughly equal numbers of seats in the legislature over a period of years, each party will have access to about the same amount of patronage. If, in contrast, one party dominates the legislature, we can assume that it also dominates patronage opportunities.

Colombia

Competition between the Liberal and Conservative parties for control of government and the spoils associated with control has structured all of Colombia's modern history. Until 1958, transfers of power from one party to the other were accompanied by large-scale turnovers of personnel and partisan violence (Solaún 1980). Public welfare suffered from the inefficiency of a bureaucracy composed of patronage appointments as well as from periodic outbreaks of violence. As early as the 1920s, critics identified the parties' excessive reliance on patronage as one of the pathologies of Colombian life. Reforms have been undertaken, however, only during two time periods, both of which correspond to periods of approximate equality between the two parties.

Colombia's first experiments with merit-oriented administrative reforms occurred during the presidency of Enrique Olaya Herrera (1930–34). The Olaya administration marked the first electoral victory of the Liberal party in the twentieth century. Olaya won by a slim margin, and the reformist Liberals' hold on government during his first two years was tenuous. Several administrative reforms were passed at this time, the most important of which aimed at improving the performance of the Ministry of Public Works and other agencies responsible for the construction and maintenance of railroads and highways (Hartwig 1983, 105–7).

By 1934 the Liberals had consolidated their electoral dominance, and Alfonso López won the presidency easily. He continued many of the new economic policies of Olaya but permitted the reassertion of partisan considerations in hiring. The Olaya administration's merit-based reforms quietly disappeared during the Liberal hegemony of 1934–46 (Hartwig 1983, 106–11).

Colombia's next attempt to establish merit as the basis for recruitment to the civil service occurred in 1958. At that time, each of the major parties controlled exactly half of the legislature and half of the available administrative appointments. This was due to a pact—the National Front—between the Liberal and Conservative parties that established parity between the two traditional rivals in the national legislature, in departmental (i.e., state) legislatures, in municipal councils, and in administrative appointments. This pact, designed to end a decade of repression and partisan violence in which more than 200 thousand people had been killed, was scheduled to remain in effect for 16 years. Each party would receive 50% of the seats in legislatures and councils. The presidency was to alternate between the two parties. A career civil service was proposed as a means of removing key jobs from partisan control, and other administrative jobs were to be distributed equally between supporters of the two parties. In short, the pact established an equal sharing of power and patronage, regardless of electoral outcomes for 16 years (Berry, Hellman, and Solaún 1980; Dix 1980; Hartlyn 1988).

The pact called for the creation of a merit-based career civil service, but presidential and legislative action were required to initiate it (Groves 1974; Morcillo 1975). Individual legislators facing this decision about whether to forego a portion of customary patronage would have to consider the electoral costs and benefits associated with patronage, as shown in Figure 2. By law, each party could claim an equal share of patronage. Nevertheless, no individual legislator could afford to eschew the use of patronage unilaterally.

Two factors contributed to making a vote for reform more attractive than it might otherwise have been. The first was the interest of all politicians in reestablishing a democratic system, which depended on ending partisan violence. Administrative reform was expected to help end the violence by providing a fair means of distributing jobs and also by contributing to better quality economic policy. Leaders in both parties had committed themselves to agreements, including complicated parity arrangements as well as civil service reform, as a way of reducing the violence and reestablishing a competitive political system. Thus, party elites had managed to forge an enforceable cooperative solution to the prisoner's dilemma of unrestrained party competition. The career interests of high-level party leaders in reestablishing the competitive electoral system explain their support for the pact. Legislators' second reason for voting for reform was the expectation that it would affect future placement on electoral lists. Party leaders in Colombia at that time could influence legislators' decisions with special effectiveness because of party cohesion and a closed list proportional representation system (see Duff 1971). Although the proliferation of factions within Colombia's two major parties during the pact subsequently reduced the influence of party leaders over legislators, in 1958 both parties were still relatively cohesive (Archer 1990).

Evidence from the Colombian case is thus completely consistent with the model. Reform occurred when access to patronage was distributed equally. Party discipline enforced by the closed list system provided legislators with an additional incentive to vote for reform.

Uruguay

In Uruguay, the career civil service was first mandated by the 1934 constitution. This constitution legalized a pact between two factions of the traditional dominant parties, the *terrista* faction of the Colorado party and the *herrerista* faction of the National, or Blanco, party.

Uruguay has historically had a two-party system within which multiple factions have independent legal status and run their own lists of candidates in elections, thus creating a de facto multiparty system. Prior to the reform, Gabriel Terra, head of the *terrista* faction, had been elected president on the Colorado ticket. In the face of severe economic distress caused by the Depression and an apparently insurmountable policy immobilism caused by Uruguay's collegial executive and powerful but factionalized legislature, Terra staged a coup d'état in 1933. Luís Alberto Herrera, caudillo of the most important Blanco party faction at the time, collaborated with Terra; and the two faction leaders entered into a pact to share government offices, excluding other factions of both parties (Taylor 1960, 23-29).

The sitting legislature was dismissed. Terra and Herrera chose a Deliberative Assembly of 99 members made up of approximately equal numbers of Colorado supporters of Terra and Blanco supporters of Herrera to act as a provisional legislature. They in turn elected a Constituent Assembly made up of Terra and Herrera supporters. The resulting constitution institutionalized the pact by mandating minority representation in the president's Council of Ministers and the equal division of Senate seats between the most-voted lists of the two most-voted parties. The division of the Senate assured these two factions equal control of appointments of all important administrative positions, the boards of directors of state enterprises, Supreme Court justices, and members of the Accounts Tribunal (Taylor 1960, 171). At the same time, it excluded other factions from access to spoils. In effect, it transformed what had been a de facto multiparty system into a two-party system with approximately equal access to patronage for both parties.

Traditionally, the Colorado party had attracted more electoral support and controlled more patronage than had the Blancos. More popular parties usually have access to more patronage, but the pact resulted in an equal division of patronage in spite of electoral inequality. In this setting, the Constituent Assembly was able to agree to establish a career civil service, which would remove some appointments from the discretion of party activists. As in Colombia, the closed list system provided faction leaders with incentives they could deploy to affect the votes of members of the Constituent Assembly.

This brief period of relative equality between two factions was unique in Uruguayan history. The two parties achieved approximate equality in electoral strength in 1962 and 1971, but the largest factions within each party remained unequal (Fabregat 1963, 6:31; Rial 1985, 12–13; Venturini 1984, 25, 50). No legislation passed to enforce the principle of meritocratic hiring in practice. Legislative and constitutional additions to the civil service law after the 1930s generally focused on providing job security, vacations, grievance procedures, and so on. In other words, later additions to the civil service laws were designed to benefit well-organized civil servants and thus contribute to electoral gains for legislators—not to require further sacrifice of electoral interests.

Venezuela

At the beginning of the democratic period in Venezuela, administrative reform seemed to be supported by everyone. Excessive corruption had helped discredit the dictator, Marcos Pérez Jiménez; and administrative reform was widely seen as needed, both to reduce corruption and to improve the state's ability to use oil revenues to foster development and increase social welfare. President Rómulo Betancourt expressed strong support for reform, foreign experts were hired to help formulate a reform and train Venezuelans to implement it, and a reform agency attached to the presidency was created (Brewer-Carías 1975a, 1975b; Stewart 1978).

The agency completed its draft plan for civil service reform in 1960, and the president submitted it to Congress. The president had a coalition majority in Congress. Since party discipline in Venezuela is strong (in part because of party leaders' control of electoral lists), prospects for reform should have been good. Nevertheless, the civil service reform bill was never reported out of committee (Groves 1967).

It continued, moreover, to languish in Congress throughout the Betancourt presidency and through that of his successor, Raúl Leoni. Venezuelan observers note that despite flamboyant public statements supporting reform, neither president really pushed the bill (Brewer-Carías 1975a, 454–57). Throughout this period, the party Acción Democrática (AD) controlled the presidency and a strong plurality in Congress. The AD leaders had much more to lose from giving up patronage than leaders of the other parties.

In 1968 Rafael Caldera of the Social Christian party (COPEI) was elected president with 28.9% of the vote, as compared with the AD-led coalition vote of 28.1%. In the legislature, the vote was split between the two largest parties, with 25.8% for AD and 24.2% for COPEI (Ruddle and Gillette 1972). The civil service reform was brought forward for consideration in Congress again, revised to make it congruent with recent constitutional and institutional changes, and passed in 1970 with support from both AD and COPEI (Brewer-Carías 1975a, 475–79; Stewart 1978, 39–40). Thus, Venezuela's first merit-based civil service law also passed during a period of temporary equality between parties.

Brazil

Getúlio Vargas established a career civil service in Brazil during the dictatorship of 1937–45. The reform had made a fair amount of headway in imposing merit as the criterion for hiring and promotion by the time Vargas was overthrown in 1945. But after the return to democracy, earlier reforms were to a considerable extent undermined (Siegel 1966, 148–75).

From the establishment of democracy until the military coup in 1964, the distribution of electoral strength and patronage in Brazil was quite unequal. The country had a multiparty system with open list proportional representation in the lower house and a majoritarian system in the Senate. The three most important parties were the Partido Social Democrático (PSD, a traditional, conservative party despite its name), the Partido Trabalhista Brasileiro (PTB, the Labor party), and the União Democrática Nacional (UDN, a middle-class reformist party). The PSD and PTB had developed from the traditional and labor wings of the political machine created by Vargas during the dictatorship (Oliveira 1973; Souza 1976). They were entrenched in the government bureaucracy prior to Vargas's overthrow, and their patronage resources remained impressive throughout the democratic period. In spite of apparently important ideological differences, the PSD and PTB formed frequent electoral alliances. The UDN, in contrast, developed in opposition to the Vargas political machine (Benevides 1981). Over the years it achieved some access to patronage as a result of entering coalitions and winning some elections, but it never equaled the other parties.

During the 1950s and 1960s, as economic development became the most important goal of the Brazilian government, concern about administrative reform reached new levels. Presidents Getúlio Vargas (during his second administration), Juscelino Kubitschek, and Jânio Quadros all proposed reforms (Graham 1968, 143–53). Public demand for reform, as expressed in the press and in answers to survey questions, was widespread. For example, a 1964 survey asked, "Which one of these do you think our country needs most: an honest government without corruption; a government that gets things done; a fair distribution of wealth; national unity; or individual freedom?" Even though the income distribution in Brazil was one of the most skewed in the world, 62% of those who answered chose "an honest government without corruption." The second-most-frequent choice was "a government that gets things done."[6] When asked to agree or disagree with the statement, "The only really important problem in Brazil is the problem of lack of character and honesty," only 15% of those who had an opinion disagreed. When asked the most important reason for their party preference, 44% mentioned honesty first, more than twice as many as mentioned party program or past record.[7]

Even in the face of such expressions of public opinion, however, legislators making the decision whether or not to vote for reform had to take into account the costs and benefits associated with patronage. Members of the UDN would have been better off if they had been able to pass a reform. Under these circumstances, it is not surprising that the UDN espoused "the struggle against the forces that have been dominant for many years of administrative corruption" (Pinto 1960) in platforms, campaigns, and speeches. Individual members of the party could still improve their chances of being elected, however, by relying on promises of patronage during the campaign.

Members of the UDN behaved as would be expected, given the costs and benefits they faced. Most members relied on patronage and deals in electoral campaigns but advocated the passage of reform bills in Congress (Geddes 1990). This stance, though rational, left them vulnerable to charges of hypocrisy from both idealists within the party and opponents outside.

In contrast, PSD and PTB legislators had no reason to vote for reform and every reason to continue relying on patronage during electoral campaigns. During the democratic period, the career civil service remained on the books; but control over new hiring returned, for the most part, to the realm of patronage. Congress reduced the status and powers of the Departamento Administrativo de Serviço Público (DASP), the agency in charge of enforcing civil service laws (Siegel 1966, 148–72). During much of the democratic period, exams were not held and appointments were made in the temporary and extranumerary (i.e., outside the merit system) categories of employment. The dividing line between merit-based career and noncareer civil servants was blurred by the passage of laws conferring career civil service status and perquisites on "temporary" employees who had not taken the exam but who had spent five years or more in public employment (Graham 1968, 140–58).

Reform laws were proposed at various times; but only two kinds of civil service laws made it through Congress: those which granted benefits to civil servants and thus involved no electoral cost to legislators and those which extended meritocratic norms into agencies controlled by one particular party and thus involved gains rather than costs for the majority of legislators. In the realm of granting benefits, Congress increased the wages of civil servants and passed several laws granting job security and higher status to unclassified employees.

The one exception to the overall decline in the merit-based civil service during the democratic period occurred when Congress extended the merit system to cover the social security institutes. Thousands of jobs in these institutes had been used during the second Vargas and Kubitschek presidencies to reward the presidents' Labor party (PTB) coalition partner (Amaral 1966, 17–19; Siegel 1963, 6). Hiring in the institutes had, in effect, been turned over to Labor party activists. By voting to include the institutes in the merit system, members of Congress from other parties could decrease the resources available to the Labor party without incurring any cost themselves. Given the unusual circumstance of the existence of a group of agencies dominated by one particular party, the vote for reform did not depend on parity in the legislature.

Chile

During its long history of democracy, Chile never passed a comprehensive civil service reform. It had no civil service commission and no uniform system of recruitment and promotion (Valenzuela 1984, 256; López Pintor 1972). It did have some requirements for entry, such as completion of the tenth grade; but even these were violated in practice. Each agency controlled its own recruitment system. As a result, some agencies were highly professionalized, others extremely politicized (Ascher, 1975, 57-86; Urzúa and García 1971, 175–78).

The Chilean party system was far more fragmented than those of the other countries discussed. Traditionally, the Radical party had greatest access to patronage. Radical party dominance began to decline in the early 1950s, but no conjuncture occurred that gave approximately equal patronage to the largest parties in Congress (Valenzuela 1985, 44–47).

Two characteristics of the Chilean democratic system further decreased the likelihood of passing a reform: the open list system of proportional representation initiated in 1958 and the fragmented party system. It might appear at first that in a fragmented party system such as Chile's, in which the "dominant" party often receives only 20%–30% of the vote in legislative elections, several smaller parties could band together to pass reforms that would deprive the largest party of its disproportionate access to patronage. In this way, a group of smaller parties with less access to patronage could improve their ability to compete against the party with the closest ties to the bureaucracy.

In an open list system, however, incumbents' interest in maintaining their advantage over competitors in their own parties outweighs their interest in depriving members of other parties of access to patronage resources. In open list systems, the candidate's place on the party list is determined by the vote the candidate receives. In other words, a candidate runs not only against candidates from other parties but also against other candidates from his or her own party.

Patronage thus becomes an even more valuable resource to the candidates who have access to it. Candidates can distinguish themselves from the candidates of other parties on the basis of programmatic appeals, offers of public goods, and ideology; but attention to casework and the distribution of private goods are among the few ways of distinguishing themselves from other candidates in the same party. Incumbents have a great advantage over other candidates in terms of their ability to distribute favors. Consequently, incumbents of all parties in an open list proportional representation system can be expected to be especially reluctant to give up patronage.

Had Chilean party leaders had an interest in reducing reliance on patronage, they might have succeeded in overcoming incumbents' reluctance to vote for reform. Despite the open list system, Chilean party leaders (in contrast to Brazilian) have substantial influence over the political careers of legislators. They influence them through control over who achieves a place on the list, whose name appears at the top of the ballot (and thus receives a disproportionate share of the votes of the unsophisticated), and who receives cabinet appointments, often a stepping-stone to executive office. Given the unequal distribution of patronage among parties and the importance of patronage to the organizational survival of parties, however, party leaders had no interest in reform.

The fragmented party system necessitated government by coalition. Agreements on the distribution of spoils among coalition partners held these coalitions together (Valenzuela 1978; López Pintor 1972, 89). Even if the president's party had been willing to make an agreement with the opposition to eschew patronage, it could not have done so because disintegration of its governing coalition would almost certainly have followed. Such an agreement with the opposition would, in effect, constitute defection in the ongoing cooperative game between the president and his coalition partners; and he could expect to be punished by loss of support for defection.

Given the fragmented party system, unequal access to patronage, open list proportional representation, and the need for coalitions in order to govern, the game-theoretic approach would predict no civil service reform in Chile; and none occurred. With regard to the occurrence or nonoccurrence of an initial reform, then, game-theoretic predictions seem to be consistent with events in all the countries examined.

The Return of an Unequal Distribution of Patronage

Up to this point, I have dealt with the situations in which the first step toward a merit-based civil service was taken. I turn now to consideration of whether these initial steps have been, or will be, followed by others. Given the notorious inefficiency of postreform bureaucracies in Venezuela, Colombia, and Uruguay, one must conclude either that civil service reform does not work in Latin America or that initial reforms were subsequently undermined.

Evidence suggests that professionalization does occur and does increase competence and public service orientations in the parts of the bureaucracy in which it occurs (Hartwig 1983; Láfer 1964; Schmidt 1974; Vieira 1967; Wahrlich 1964) but that initial reforms affect only a small part of the bureaucracy. Where conjunctures favoring the passage of reforms have lasted only a short time, additional increments of reform have not followed initial reforms. Further, subsequent legislation and executive decrees have sometimes vitiated earlier reforms.

In Venezuela, when the AD returned to its then-customary dominant role with the election of Carlos Andrés Pérez in 1973, nonpartisan administration suffered a setback. Pérez won the presidency with a strong plurality (48.4%, compared with 36.7% for COPEI). He was the first elected Venezuelan president to have an absolute majority in both houses of Congress (Karl 1982, 182–84). He asked for—and eventually got—special powers to enact by executive decree a package of proposals aimed at controlling and using effectively the windfall of oil money threatening to engulf the nation. Included among these projects were several administrative reforms. Pérez expressed strong support for administrative reform, including professionalization of personnel. His actions, however, tended to belie his words.

Decree 211, issued by Pérez, allowed the administration to increase the number of nonclassified public employees (i.e., temporary and low-status employees who need not pass exams to enter the service), as well as the number of positions *de confianza* (high-status appointments in which loyalty is considered an appropriate criterion for recruitment). Employees *de confianza* in 1982 included all division chiefs, those employed in fiscal sections, buying, supplies, and document reproduction, and all secretaries in these areas. COPEI claimed that 80 thousand people had lost their jobs for political reasons during the administration turnover when Pérez came to power (Karl 1982, 267). Pérez, in other words, took the opportunity provided by having a majority in the legislature to build the political machine that would form the foundation of his second successful campaign for the presidency (Geddes n.d., chap. 6).

Some administrative reforms have occurred, however, since the Pérez administration. An anticorruption law, for example, was passed in 1982 when AD and COPEI each received 39.7% of the vote in legislative elections.[8] The problem of professionalizing personnel, however, remains unsolved (Brewer-Carías 1985).[9] Partisan considerations still affect most hiring decisions, and turnover in the bureaucracy when a new administration comes into power is so high that administrative output falls noticeably (Cova and Hannot 1986). Corruption continues to be a serious problem.

The situation in Uruguay resembled that in Venezuela, but in a more extreme form. After the end of the pact between Terra and Herrera, Uruguay never again experienced equality among the largest party factions. The 1934 constitution mandated that hiring and promotion be based on merit but did not establish an agency to conduct exams. Instead, each bureaucratic entity set its own standards. Some evidence about the strictness of these standards can be inferred from education statistics. Of public employees in the mid-1950s 46% had not finished primary school, and 70% had completed 10 or fewer years of schooling (Taylor 1960, 215–19). Entrance exams in many agencies guaranteed little more than literacy. There was one important barrier to entry, however; applicants were not permitted to take the test unless it was signed by the neighborhood *sublema* (i.e., party faction) boss (see Biles 1972, 1978).

Most legislation regarding the civil service and additional provisions added to subsequent constitutions dealt with job security and grievance procedures. These issues involve no cost to legislators as long as the number of jobs keeps expanding and brings them electoral benefits in the form of support from government employees (estimated at 27.6% of the working population of Uruguay in 1956; see Taylor 1960, 100).

As a result of this series of laws and constitutional provisions, by the 1960s it had become virtually impossible to dismiss government employees. The 1952 constitution provides that the Senate must approve the dismissal of a classified employee. Even the dismissal of temporary employees has led to serious political repercussions (Taylor 1960, 215). As noted, such a large proportion of agency budgets was spent on wages that at times, agencies could not afford to buy equipment needed in order to carry out their functions and supply government services.

To summarize, in Uruguay's factionalized political system, legislators supported the elements of career civil service that could be converted into electoral advantage, especially job creation and job security. They did not provide for the imposition of merit as the criterion for hiring and promotion, which would have reduced the ability of factions to service their clients.

The pattern of implementation of the 1958 reform in Colombia resembles that in Uruguay. Once parity between the two major parties was established, competition among the factions within each party for shares of the party's half intensified. Struggles for patronage among factions of the same party became increasingly vitriolic over time. As in Uruguay, the party pact has led to a sharp increase in the number of government jobs.

All Colombian presidents during the early democratic period made some effort at administrative reform, and quite a few reforms gained legislative approval. Merit-based hiring has been an exception, however. By 1966 when Carlos Lleras Restrepo came to power, only 5% of public employees were included in the merit-based career civil service. Lleras mounted an aggressive campaign against corruption and patronage and for administrative reform. Congress, however, successfully blocked his proposed personnel reform. Unable to extend the merit system to cover more jobs, near the end of his term Lleras issued a decree allowing public employees to enroll themselves in the career service without taking the exam, thus undermining the meritocratic element of civil service.

As the party pact neared its end, conflict between the parties increased and interest in administrative reform waned, even in the executive branch. With the failure to extend the merit system, civil service in Colombia, as in Uruguay, had become synonymous with inflexibility and the inability to fire incompetents (Hartlyn 1988). Misael Pastrana, the last president during the pact, showed little interest in reform, being more interested in consolidating his party's position before the first election unfettered by parity agreements. His successor, Alfonso López Michelsen, suspended the career civil service during a state of siege.

The general assessment by observers of the current Colombian scene is that professionalization of some key sectors of the Colombian bureaucracy has occurred but personnel reform in general has failed (Hartwig 1983; Vidal Perdomo 1982). In spite of considerable presidential support, campaigns against corruption in the press, and supportive public opinion, legislators in the factionalized party system have not found it in their own interest to extend the merit system. Since the end of the National Front, party parity has not occurred. Because of the ease of forming faction lists, party leaders failed to exert much influence over legislators' votes (Archer 1990).

In brief, then, in all the countries examined, reforms initiated during periods of party equality or during a dictatorship suffered reverses when the distribution of power among the competitive parties became unequal. Even in the United States, when the election of 1896 resulted in renewed Republican dominance, the merit system was seriously threatened. President McKinley removed the exam requirement for ten thousand jobs. Further, during the McKinley administration Congress passed legislation excluding thousands of new appointments from the system (Van Riper 1958, 171–75). If McKinley's assassination had not brought Theodore Roosevelt unexpectedly to the presidency, administration in the United States might look less different from Latin America than it does today.

Civil service reforms generally include two kinds of provisions: (1) requirements for merit-based hiring and promotion and (2) guarantees to employees of job security, fair treatment, union representation, and so on. In unequal or fragmented party systems, legislators have been reluctant to increase the number of jobs included in the merit system, since each new inclusion reduces the resources available to politicians and party leaders for use in their struggle with each other. Laws extending perquisites and job guarantees to larger numbers of employees have posed no problem for legislators, however, since they bring electoral benefits from grateful employees.

Implications for the Future of Reform

In the United States, where both parties enjoyed similar access to patronage from 1882 until 1896, civil service was gradually extended "by executive order, taking advantage of feeble statutory authorization" (Schattschneider 1942, 138). By 1896 the merit system had been extended to cover about half of all appointments, which included "the bulk of the offices which it was then either legal or politically and administratively practical to place under the merit system" (Van Riper 1958, 130). Most of these extensions occurred when the party in control of government had lost an election and expected the incoming party to dismiss its supporters (Skowronek 1979). This pattern of incremental extension was possible because, in a system of two approximately equal parties, occasions recur when it is temporarily in the interest of one party or the other to extend the merit system. Over time, this series of instrumental decisions creates a professional civil service.

The same thing is likely to happen in South America but will take longer because of differences in party systems. The initial establishment of a merit-based civil service, an agency to administer it, and (usually) a school to train civil servants creates islands of competence within the bureaucracy and concentrates advocates of further reform strategically inside government. Though they lose many battles, they rarely disappear from the scene completely.

Reform continues to be strenuously advocated both within the executive branch of government and in the press. Elected officials, however, often feel reluctant to extend reform. Even in the two countries with two-party systems, the institutionalization of factions that run separate electoral lists has transferred the struggle for patronage from a struggle between parties into a struggle among factions within each party. This makes it extremely unlikely that equality can be maintained for any length of time, since it must be maintained among factions, not just parties.

Multiparty systems in Latin America have so far not produced lasting periods of relative equality between the two most popular parties or coalitions. One party has usually tended to dominate, and it has not served that party's interest to extend the merit system. If two approximately equal parties were to emerge as the only serious competitors for power in a multiparty system, however (as seems quite possible in Venezuela), the game-theoretic model would predict further extensions of reform; and some extensions have occurred in Venezuela.

Conclusion

The very simple game-theoretic model proposed implies two predictions about when administrative reform should occur: (1) reforms are more likely to pass the legislative hurdle when patronage is evenly distributed between the strongest parties, and (2) initial reforms are more likely to be followed by further extension of reform where the electoral weight of the two top parties remains relatively even and stable.

The rational actor assumptions on which the model is based also imply several predictions about the effects of certain institutions on the probability of reform. Open list proportional representation, for example, because it makes patronage a valuable resource to incumbents in their struggle against challengers within their own party, reduces the probability of reform. Electoral rules that result in the proliferation of candidate lists (e.g., minimal requirements for party qualification or easy formation of dissident electoral lists within parties) also reduce the probability of reform because fractionalization reduces the probability of an equal distribution of patronage among the larger parties. Institutional features of the party system that give party leaders more influence over legislators (such as control over placement on the list) can work in either direction. Where party leaders have an interest in reform, their ability to impose party discipline increases the probability of reform; but where party leaders have no interest in reform, their influence makes reform less likely.

The evidence I have examined has proved consistent with these predictions. All the instances of initial civil service reform in democracies occurred during periods of party parity with regard to patronage. Reforms did not occur in democracies with open list proportional representation. Factionalism seemed to undercut the ability of two-party systems to produce recurrent situations of equality. Finally, when the parties returned to their normal situation of inequality after a temporary period of parity, reforms were not extended and, in fact, were often cut back.

Moreover, evidence from these cases has proved inconsistent with other explanations of reform. It is, for example, sometimes suggested that administrative reform occurs when countries attain a level of economic development that makes the continuation of government incompetence economically costly. The dates of initial reforms in these cases, however (Uruguay 1934, Brazil 1937, Colombia 1930–34 and 1958, Venezuela 1970, and Chile none prior to the 1973 Pinochet regime), offer little support for a direct link between development and reform. A related but more political argument hypothesizes that reforms occur when the demographic changes that accompany development give reformist parties supported by middle-class and manufacturing interests the chance to defeat traditional machine parties. Latin America offers few examples of victories by unambiguously reformist antimachine parties. The Christian Democratic Frei administration in Chile comes closest to what North Americans think of as a reform government. It did not introduce civil service; rather, the many administrative changes initiated by the Christian Democrats sought to monopolize offices for their own party. In contrast, the reforms that actually introduced meritocratic hiring (as demonstrated in the case histories) occurred when traditional machine parties found themselves forced to share power with other traditional machines.

In contrast to the arguments advanced to explain reform in the United States, Latin American specialists have sometimes suggested that the Iberian colonial heritage shared by Latin American countries predisposes them toward clientelism and against impersonal procedures such as meritocratic recruitment to civil service. There may be some truth in this argument, but it obviously cannot explain the very considerable differences among Latin American cases.

The game-theoretic model has thus proved sufficiently useful to deserve further research. Claims about its generality have to be somewhat cautious because of the small number of culturally and historically similar cases examined here. It may be that the domain of this model is limited to the Western hemisphere. Nonetheless, its implications are quite far-reaching. It suggests that administrative reforms will be difficult to achieve and maintain in democracies, especially democracies with fragmented party systems. Certain characteristics that are often thought of as increasing representativeness, such as multiparty systems that reflect a wide spectrum of interests and open list proportional representation, may paradoxically cause elected officials to be less responsive to the public interest.

One of the promises of democracy is that it makes government services available to all citizens regardless of wealth or status. The failure to professionalize public administration, however, makes that promise hard to keep. Stories abound in Latin America about the need for bribes or pull in order to get everyday services such as renewal of a driver's license. More seriously, inefficiency and incompetence in government agencies can be so extreme that clients' needs cannot be served at all. For example, Montevideo's *El País* reported in 1960 that the Fund for Pensions for Rural, Domestic, and Aged Workers was up to two years behind in the commencement of payments to nearly four thousand people (Taylor 1960, 222). During the democratic period, Chileans eligible for pensions routinely sought the help of elected officials to avoid the months or even years of red tape involved for the politically unconnected to initiate payments (Tapia-Videla 1969, 300–13; Valenzuela 1977, 120–37). All but one of Brazil's many social security institutes had gone bankrupt—in part because of excessive employment of untrained Labor party supporters—by 1964 (Malloy 1979). In these instances (and many others that could be cited) the failure of public service directly affects the quality of life of the ordinary people whom democracy is supposed to benefit.

It is ironic that the reforms that would improve efficiency and fairness in the provision of government services should be impeded by the same representative institutions whose manifest purpose is to reflect constituents' interests.

Notes

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1. Although bureaucratic incompetence and corruption also plague authoritarian regimes, I shall examine only democracies, because the incentives that determine whether political leaders will initiate reform depend on institutional features of the political system. Consequently, one would need a different model to explain reform in an authoritarian institutional setting.

2. See nn. 6, 7.

3. Presidents may appear to be an exception to the assertion that politicians care most about reelection, since, in most Latin American countries, they cannot be immediately reelected. They can, however, serve again after one or two terms have elapsed. Many do so (e.g., Fernando Belaúnde and Carlos Andrés Pérez); and more hope to but are prevented from doing so either by military intervention (e.g., Eduardo Frei and Juscelino Kubitschek) or by ambitious competitors within their own parties (e.g., Carlos Lleras Restrepo). The incentives presidents face as they confront the reform issue are detailed in Geddes n.d., chap. 6.

4. Speaking of majority parties in the Latin American context involves a degree of simplification. In fact, legislative majorities are usually coalitions. This should not, however, affect the logic of the argument.

5. The categorization of Uruguay as a democracy at the time of the reform is somewhat dubious. The elected president had staged a coup and replaced the elected legislature. Still, two factions continued to function as the most important political competitors. Since party competition continued during this period of modified democracy, Uruguay was retained in the small universe of democracies (Taylor 1952).

6. This question comes from a survey conducted by the United States Information Agency in March 1964 (World Survey II: Attitudes toward Domestic and Foreign Affairs [N = 466]), made available by the Inter-University Consortium for Political and Social Research (ICPSR 7048), University of Michigan.

7. These questions come from a survey conducted by Júlio Barbosa et al. (Political Behavior and Attitudes in a Brazilian City, 1965-1966 [N = 645]), made available by the Inter-University Consortium for Political and Social Research (ICPSR 7613), University of Michigan.

8. Ley Orgñica de Salvaguarda del Patrimonio Público (*Gaceta Oficial*, no. 3077, *extraordinario*, 23 December 1982). My thanks to Michael Coppedge for bringing this law to my attention.

9. "Burocracia: Ciudadano" [Bureaucracy: Citizen], in *El Diario de Caracas*, 2 August 1987.

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NATIONALISM VERSUS ETHNIC IDENTITY IN SUB-SAHARAN AFRICA

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Part and parcel of the conventional wisdom about rural publics in Africa is that populations on the periphery will accord ethnic solidarity greater significance than national consciousness. A survey of neighboring Hausa villages on different sides of the Niger–Nigeria boundary counters this myth. Probing issues of self-identity and ethnic affinity, we found that most Hausa villagers on the frontier did not place their Hausan ethnic identity above their national one as citizens of Nigeria or Niger and expressed greater affinity for non-Hausa cocitizens than for foreign Hausas. However, expressed attachments to ethnic, national, and other social identifications (such as religion) varied according to village: citizenship does make a difference in the political consciousness of villagers on the geographic margins of the state. More survey research in other trans-border regions should shed further light on processes of state penetration and national integration in developing countries.

Throughout the last decade or so, studies of rural publics in Africa have challenged the conventional wisdom that populations on the periphery will accord greater significance to ethnic solidarity than to national consciousness. Hayward's (1976) investigation of Ghanaians' level of political information is a case in point. Surveying one urban and five rural communities throughout Ghana in 1970, Hayward dispels the (usually implicit) assumption that "the masses have little political knowledge and that they are generally ignorant of the political process, the national leadership, political organizations, the national territory and its people" (1976, 433). Hayward comes to this conclusion after gauging the level of information concerning political actors and organizations, knowledge of national territorial limits and, of most interest to the present study, identification with cocitizens of different ethnic groups (p. 434).

Barkan (1976) takes Hayward's thesis another step, examining political knowledge and voting behavior among the electorate of rural Kenya. In this study (based on surveys in 13 parliamentary constituencies), Barkan argues that "citizens living in the rural/traditional sector appear to be more integrated into the national political system, and to be more rational participants in that system than students of political development usually assume" (p. 454). In a by-now classic spatial image portraying the relationship between elite and nonelite, Barkan writes:

> Peasants residing on the periphery of [peasant] societies may in fact be "citizens" of the new nations emerging within their societies' boundaries despite parochial and traditional settings in which they live. . . . Rarely do we ask how those on the periphery are already attempting to penetrate the center. . . . Perhaps it is time to . . .

> [take] a careful look at what is happening on the boundaries *vis-à-vis* the center of these polities. . . . As the questionable assumptions of the "conventional wisdom" make painfully clear, we have probably neglected an important part of the story. (pp. 452, 455)[1]

Clearly, Barkan is using the familiar center-periphery analogy in the metaphoric sense. Taken literally, in a geographic sense, however, it does encompass another piece of conventional wisdom: that outlying, remote, rural frontier communities in developing countries identify relatively little with the "new nations" within whose boundaries they have been boxed (especially in contrast to urban centers). This is particularly the case when these boundaries (invariably a product of European colonialism) have arbitrarily divided discrete ethnic groups into separate states. Since Africa presents multiple instances of ethnic partition, the effects of border division on African rural communities' political knowledge (particuularly national, as opposed to ethnic, identification) are of special interest.[2] Hitherto, neither the quantitative survey analyses of rural publics and electorates (Barkan 1976; Hayward 1976; Niemi and Barkan 1987) in Africa nor the ethnographic descriptions of Africa's state-split ethnic groups (Asiwaju 1985) have considered this "partition factor" upon political identity in a quantitatively verifiable way. The present study is an attempt to do just that. It presents three findings:

1. Rural populations—even in communities located on the geographic margins (frontiers) of the state—are more highly politically conscious than has been supposed. Such consciousness is demonstrated by an expressed degree of national identity, which among most residents of the surveyed locale is stronger than ethnic identity.
2. Even where the rural, frontier communities in question are ethnically identical and have been partitioned, residents express greater affinity for ethnically different cocitizens than for coethnics of different nationality.
3. The above tendencies notwithstanding, the *degree* of national vis-à-vis ethnic consciousness does vary in different countries, a finding consistent with the distinctive national experiences emanating from the consequences of partition.

At a time when most scholars of Africa regard the state as a weak and declining institution, these findings suggest that it does retain a special importance, at least in regard to the development of national consciousness.

We shall now present the results of surveys conducted in neighboring villages of the same ethnic profile but divided by an international boundary. The ethnic group is the Hausa of West Africa. The two states are Nigeria and Niger.[3]

Setting and Background

Yardaji on Nigeria's northern boundary (Katsina State) and Yekuwa on Niger's southern border (Zinder Province) are eight miles apart. Each settlement has approximately three to four thousand inhabitants. Like most villages along the Nigeria-Niger boundary, the inhabitants of Yardaji and Yekuwa speak Hausa, follow Islam, and practice rainfed agriculture. Though there is no road connecting the two communities, there is significant social and economic intercourse between them. Each is endowed with a school, an infirmary, and a weekly market. Both occupy roughly equivalent rungs on the hierarchical ladder of their respective state's administration: Yardaji is a village area headquarters in Nigeria, and its chief is a village area head; Yekuwa and its chief are functional equivalents, even though the administration of Niger does not have an exact correspondence to the village area and village area head as found in Nigeria.

Elsewhere (Miles 1986), preliminary findings from pilot surveys conducted in the two villages in 1983 suggested a higher level of expressed national consciousness in Yekuwa as opposed to Yardaji.[4] This was attributed in large part to a longer and more authoritarian history of military rule in Niger. Concerning self-identity, the Yekuwa villagers were found to attach more importance to their identity as citizens of Niger than Yardaji villagers did to their identity as citizens of Nigeria. In terms of ethnic affinity, Yardaji villagers seemed to feel closer to Hausas of Niger than Yekuwa villagers did toward Hausas of Nigeria. In other words, in Nigeria, Hausa villagers felt less affinity for non-Hausa countrymen (e.g., Igbo and Yoruba) than Hausa villagers in Niger did for their non-Hausa cocitizens (e.g., Zarma and Tuareg).

By the summer of 1986, when the present survey was conducted, Nigeria itself had experienced two-and-a-half years of military rule under two separate regimes (Buhari and Babangida). More importantly, the border between the two countries was closed for most of this time, as Nigeria deployed its various immigration, customs, and security forces to keep foreigners from entering the country. This circumstance alone may have heightened Yardaji consciousness of being citizens of Nigeria and mitigated the differing levels of national consciousness between the two villages. Indeed (as will be demonstrated below) Yardaji-ites were, in 1986, more likely to identify with non-Hausa cocitizens than the pilot survey of 1983 would have indicated, appreciably diminishing the difference between them and Yekuwa villagers noted at that earlier time. Nonetheless, the findings for the self-identity questions indicate that important discrepancies in levels of national consciousness within the two communities still remain.

Methodology

The first stage of the survey required original maps, indicating discrete households, to be drawn for each village. In Yardaji, approximately four hundred discrete households were mapped and listed; in Yekuwa, there were approximately five hundred separate households. Every fourth household in Yardaji was selected for the survey, and every fifth household in Yekuwa, for a total sample of one hundred in each village.

African social structure in general, and Hausa society in particular, requires that information relating to the household be conveyed only by its senior member. Most typically, this would be a male (husband and father), although in a few instances single women (usually widows) had become heads of households and were accordingly polled. Only when the official head of household was absent for an extended period of time (say, a pilgrimage to Mecca) was the second senior member of the household surveyed.

Following Hill (1972, 88–91, 148) and Paden (1973, 36–40), a profile of the traditionally important attributes of Hausa identity was developed; this was expanded by including more "modern" attributes (i.e., those arising from colonial and postcolonial administrative and political structures). Seven items, specifically, were stated for relative prioritization by the respondent. These were birthplace, village residence (Yardaji or Yekuwa), country (Nigeria or Niger), province (Kaduna[5] or Zinder), religion (Islam), ethnic group (Hausa), and subethnic identity (Daura Hausa). Since three-quarters of the respondents were born in their villages, most had only six identity variables to choose from. Each of the six (or seven) identity variables was put on a separate index card (written both in Roman and Arabic [ajami] script) and laid out on the ground or a mat in front of the respondent.

Descriptions of Samples

The basic profile of the two samples varied relatively little. A few more current Yekuwa-ites than Yardaji-ites actually were born elsewhere (18% vs. 13%); approximately the same number of migrants in each village had come from across the border (6% vs. 5%). Interestingly, virtually half the respondents in each village claimed that their ancestral home (the area or place from which their ancestors originated) was now situated across the international border (47% for Yardaji and 50% for Yekuwa). This indicates a significant degree of early colonial or precolonial migration, which is consistent with the recorded history of the area.

In terms of occupation, virtually all men in both villages are farmers, so only secondary occupation responses were recorded. In both samples, 17% gave their secondary occupation as *Islamic students* or *practitioners; merchants* came second, 15% in Yardaji versus 9% in Yekuwa. With the earth in and around Yardaji red, hard, and claylike, *potters* constituted 13% of the Yardaji workers. In Yekuwa, third place went to *butchers*, with 8%. Twice as many respondents in Yekuwa—a full quarter of the sample—had no secondary occupation whatsoever compared to respondents in Yardaji.

Another descriptive characteristic is "life station," which refers to a socially ascribed status combining age, marital, and parental status, as well as work force participation. This attribute actually varies in meaning from one village to another. For example, while in Yekuwa a recently married young man might still be called *samari* (youth), in Yardaji such an individual would always be considered by fellow villagers to be a *mai-gida* (householder). Adopting the statuses as ascribed in each locale, then, in Yardaji 28% of the respondents were categorized as "young heads of household," 40% as "early-middle-aged," and 23% as "late-middle-aged." In Yekuwa the percentages were 37%, 37%, and 19%, respectively. (The proportions of "unmarried youth" and "elderly" were 2% and 7% in Yardaji, 1% and 5% in Yekuwa.)

More notable differences were found for two remaining sample characteristics. Yardaji respondents had received more education, especially religious instruction: 85% had studied or were still studying the Koran, as opposed to 72% in Yekuwa. In both samples, just 3% had received only a secular education. Whereas 5% had received no education, religious or secular, in Yardaji, this proportion reached 20% in Yekuwa.

Yardaji-ites were also slightly more traveled than Yekuwa-ites in general, having resided elsewhere (defined as living in another village or town for at least a month) 84% versus 77%. However, Yekuwa-ites had more international exposure: a full 50% of them had resided outside their country (mostly in Nigeria), as opposed to only 23% for Yardaji villagers. (This excludes travel to Mecca for the pilgrimage.)

A more extensive multivariate analysis, ideally with a larger sample, would be needed to assess how such variables as education, foreign living, and travel experience (for which residents of these two villages vary as stated) may join with other sociopolitical factors to produce the specific intervillage identity differences uncovered by this study. More important for present purposes, however, is that impressive opinion commonalities in Yekuwa and Yardaji prevail despite these population discrepancies.

Aggregate Findings

Within the two-village sample taken as a whole, Islam provided the strongest identity, being the first chosen by 88% of respondents. Village residence stood second in importance, with nearly half placing it among their highest two identity preferences. Not until we look further down the scale does the comparison between ethnic and national consciousness emerge clearly. As one of the first three choices, national identity (*country*) was picked by 44% of all respondents. By contrast, ethnic identity (*being Hausa*) was ranked in the top half of identities by only 33% of respondents. Thus, once the primordial attachments to religion and root local community (*village*) are expressed, there is a definite (albeit moderate) overall preference for national, as opposed to ethnic, identity. As for the least salient identities, province (in Nigeria *state*, in Niger *department*) fell last among the total six identity choices applicable for all respondents.

Given what we know about the powerful role of Islam in Africa, it is not surprising to come upon religion as the most salient identity. Nor does the strong attachment to village come as a surprise in a society that places a premium on loyalty to one's home community and where there is such a strong psychological sense of place. Despite seasonal migration and, in some instances, long-term urban relocation, few in rural Africa ever completely sever this physical and emotional tie. The primacy of Islam and the importance of home community (i.e., village residence) established, we then cross into more interesting analytic terrain concerning the respective roles of ethnicity and nationalism. That national identity in the Nigeria–Niger border region of Hausaland is at least as important as ethnic identity makes plain that recent reports of the death of the African state are indeed premature.

Comparative Analysis

Though the attribute *being Muslim* was of overwhelmingly greatest importance to villagers on both sides of the border, there were significant differences ($p < .05$) in the relative weighting of Islamic identity between the two surveyed villages (see Figure 1). In Yardaji, 96% of all respondents chose Islam as their first-ranked social identity, while in Yekuwa the corresponding percentage was 81%.

Village residence appeared as the single most common response for choice of second-ranked social identity in both villages, 43% in Yardaji and 37% in Yekuwa. About a quarter of the population in each village chose country as their second-ranked identity. Beyond this, responses for this survey item were distributed across all five remaining identity categories for these bordering villages, with an overall significant divergence in the resulting pattern ($p < .05$).

Following from the cross-village differences already seen for these first two questions, there were significant variations between Yekuwa and Yardaji residents for most of the remaining third- through seventh-ranked social identities. The two exceptions were fifth-ranked social identity and seventh-ranked social identity, of which the latter was declared only by the 31 respondents whose birthplace was other than current village residence.

Subethnic group (i.e., being of the Daura Hausa "family") is clearly more important for the residents of Yardaji. It first surfaces as a third-place priority for 32% of the Yardaji samples but only 11% of the Yekuwa samples; as a fourth priority, it is claimed by 33% in Yardaji and 13% in Yekuwa. Historical reasons, both precolonial and colonial, undoubtedly contribute to this variation. For the moment, suffice it to say that as a result of the 1906 Anglo-French boundary demarcation, Yardaji was restored to an emirate bearing the name of its ancient ancestry (Daura Emirate) and today also forms part of a similarly sounding Nigerian government administrative unit (the Daura Local Government Area). In contrast, the colonial partition reified Yekuwa's

Figure 1. Social Identity Rankings by Village

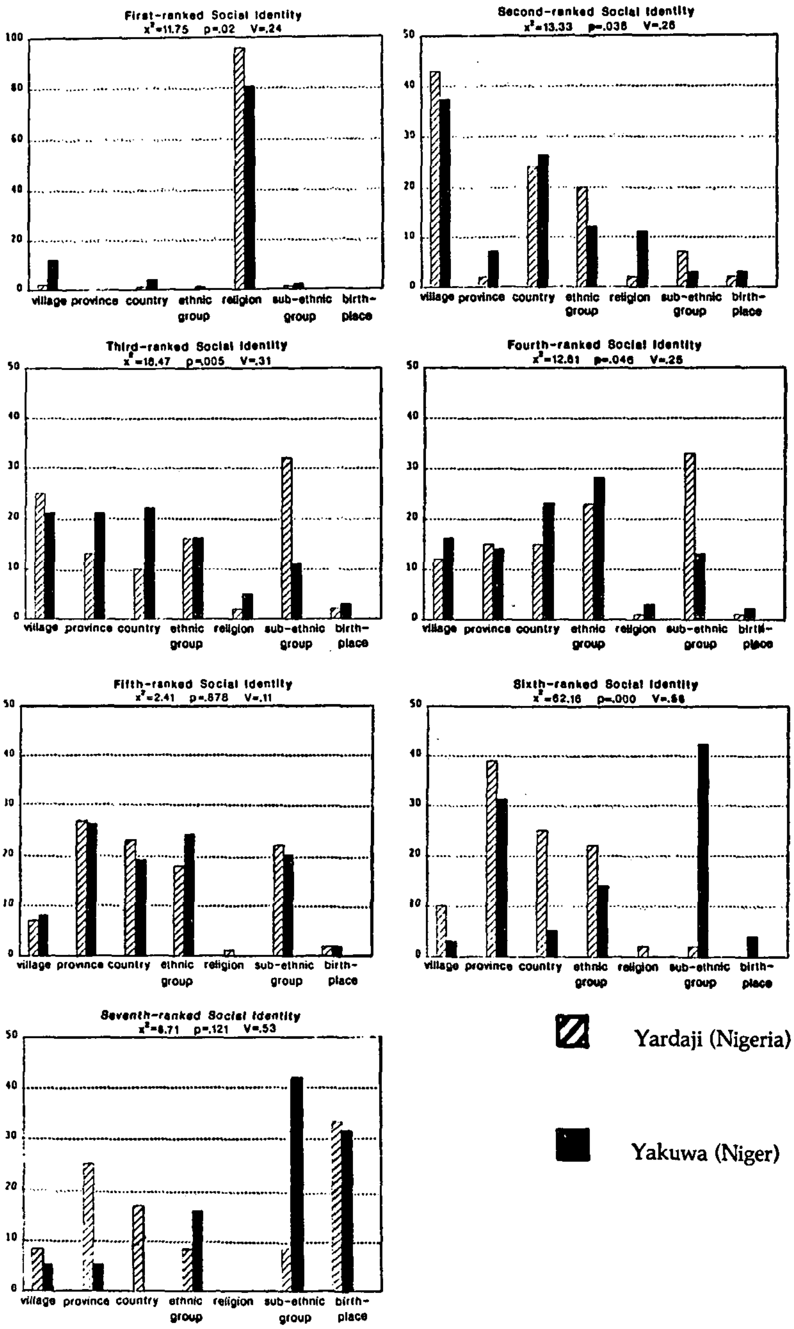

Table 1. Social Identity Rankings: Summary Village Comparison

Identity	Weighted Mean Value		Relative Ranking		Between-Village Chi-squared[c]	Significance
	Yardaji[a]	Yekuwa[b]	Yardaji	Yekuwa		
Religion	1.23	1.30	1	1	12.35	.03
Village	3.13	2.84	2	2	12.33	.06
Subethnic group	3.80	5.06	3	7	62.04	.00
Ethnic group	4.11	4.18	4	4	6.70	.35
Country	4.21	3.42	5	3	24.83	.0004
Birthplace	4.73	4.95	6	6	2.75	.74
Province	4.98	4.56	7	5	6.62	.25

[a] Sample size for all rankings other than birthplace is 100 and for birthplace is 11.

[b] Sample size for all rankings other than birthplace is 99 and for birthplace is 20.

[c] The chi-squared statistic in this situation is being used to compare the overall distribution of rankings for each identity item in the two villages. Both for this analysis and the preceding comparison of first- through seventh-ranked social identities by village, the chi-squared calculation for certain tables is undermined by low expected cell frequencies. For consistency's sake and because of the importance of all identity categories to the study design, however, no categories were merged or eliminated.

detachment from Daura suzerainty, placing it in Niger.

Turning to national-versus-ethnic consciousness, interesting village differences are found. Niger is mentioned in Yekuwa as a first or second priority, only slightly more than is Nigeria in Yardaji. However, country emerges as the most common third preference in Yekuwa much more frequently than in Yardaji (22% vs. 10%). In the follow-up position (fourth priority), this differentiation is replicated (23% vs. 15%).

Prioritization of ethnic consciousness is less clear. A negligible number of persons in both villages selected ethnicity as a first-ranked social identity. However, as a second priority, "Hausaness" is chosen 20% of the time in Yardaji but only 12% in Yekuwa. It is chosen equally as a third position (16%); but again, at this level, the national factor is higher than the ethnic one in Yekuwa (22% vs. 16%), while the obverse is true for Yardaji (16% choosing Hausa, and 10% Nigeria).

To derive one aggregate measure of the importance of each of the seven social identities in Yardaji and Yekuwa, a weighted average was calculated (which requires treating these ordinal data as continuous). Table 1 shows that religion, village, and birthplace occupy the same position in both villages. The greatest divergence (as noted) occurs for subethnic group, ranked third in Yardaji but seventh in Yekuwa.

Of greatest theoretical interest to this study are the social identities of ethnic and country affiliation. Table 1 provides both relative and absolute ranking information for these items. The former refers to the priority accorded these identities *within* each village vis-à-vis the other six listed identities. The latter signifies the actual numerical weight generated for these identities, irrespective of relative standing within the two communities. For country, both the relative (third vs. fifth place) and absolute (3.42 vs. 4.21) indicators were higher in Yekuwa than in Yardaji. Yardaji and Yekuwa gave the same priority (fourth place) to ethnicity in relation to all seven identity choices—although at this position ethnicity was above country in the former village and below it in the latter. Numerical values for ethnicity were close for the two sample groups. Several conclusions follow: (1) national consciousness is measurably greater in Yekuwa than Yardaji; (2) Hausa ethnic consciousness seems slightly stronger in Yardaji, at least in the sense that it has a higher standing with respect to country there; and (3) Hausa identity does not submerge nationalism in either Yardaji or Yekuwa.

These findings are consistent with the 1983 survey findings (Miles 1986), and may be explained by the reasons then suggested: a stricter, more intrusive colonial experience in French Niger than British Nigeria; a more markedly military-nationalist postindependence regime in Niger than in Nigeria (itself characterized by spates of civilian rule and democratic politics); greater ethnic diversity—hence consciousness—in Nigeria[6]; a relatively large population and inefficient bureaucratic apparatus, limiting governmental penetration into local areas, in Nigeria; and a laissez-faire Nigerian societal ethos, in contrast to the more rigidly controlled *société de développement* of Niger. In short, separate colonial and postcolonial experiences are consistent with distinctive levels of national and ethnic consciousness among ethnically identical (but internationally partitioned) neighboring Hausa rural frontier communities.

Ethnic Affinity

The second part of the survey gauged villagers' relative affinity to specified non-Hausa ethnic and non-Muslim religious groups within their own country as another way of comparing the strength of national identity against the strength of ethnic identity for these rural populations. The question posed (in Yardaji, for instance) was, "Whom do you feel closer to—the ______ of Nigeria or the Hausas of Niger?" Four groups—Fulani, Bouzou, Maguzawa, and Kirista (Christians)—were common to both sets of respondents. In addition, respondents of Yekuwa were asked about Tuaregs and Zarmas (two important minorities in Niger) and those of Yardaji were queried about the Igbo and Yoruba (the other two major groups in Nigeria).

Table 2. Affinity Choices for Non-Hausa Cocitizens As Against "Foreign" Hausas (in percentages)

Non-Hausa Cocitizens	Yardaji Nigeria	Yekuwa Niger
Fulani cocitizens	86.7	79.8
Foreign Hausas	13.3	20.2
Bouzou cocitizens	72.7	73.7
Foreign Hausas	27.3	26.3
Maguzawa cocitizens	69.0	58.6
Foreign Hausas	31.0	41.4
Kirista cocitizens	45.0	23.2
Foreign Hausas	55.0	76.8
Yoruba cocitizens	65.7	—
Foreign Hausas	34.3	
Igbo cocitizens	61.0	—
Foreign Hausas	39.0	
Tuareg cocitizens	—	59.6
Foreign Hausas		40.4
Djerma (Zarma) cocitizens	—	77.8
Foreign Hausas		22.2

For all but one of the four groups resident in both countries (that of the Kirista), a clear majority (59%-87%) of the respondents in both villages expressed closer affinity to their non-Hausa cocitizens than to Hausa from across the border (see Table 2). Yet the degree of affinity varied according to cultural distance from the Hausa. The Fulani were most uniformly perceived as closer to the respondents than Hausas from across the border. To a slightly lesser degree, cocitizen Bouzous (*bugaje*)—slave descendants of the Tuareg—were also solidly preferred to foreign Hausas.

The Maguzawa present an interesting case of group differentiation from the Hausa. Although ethnically indistinguishable from the Hausa, the Maguzawa do constitute a distinct group by virtue of their "pagan" (i.e., non-Muslim and animistic) background. Even when

Table 3. Cumulative Summary of Affinity Choices for Non-Hausa Cocitizens versus "Foreign" Hausas

	Yardaji		Yekuwa		Both Villages Combined	
Preferred Group	N	%	N	%	N	%
Non-Hausa cocitizens	397	66.6	369	62.1	766	64.4
"Foreign" Hausas	199	33.4	225	37.9	424	35.6
Total	596	100.0	594	100.0	1,190	100.0

Maguzawa have individually converted to Islam, as most have done, the Hausa continue to see them as different from themselves. While still preferred by a majority to Hausa from across the border, the status as converts to Islam probably mitigates affinity for a sizable group of respondents.

The religious factor becomes even more pronounced when considering the case of the Hausa Christians (Kirista). Only here did communal (i.e., religious) sentiment overcome national affinity: a majority in each village said they felt closer to Muslim Hausa foreigners than to Christian (albeit Hausa) compatriots.

Complexities of the relationship between religion and group acceptance or rejection by Hausa villagers become evident when we turn to respondents' preferences concerning country-specific ethnic groups. Many Yoruba in Nigeria are not Muslim; very few Igbo are Muslim. Still, Yardaji villagers stated that they felt closer to both groups than to the Hausa of Niger. As to ethnic groups specific to Niger, the Tuareg (a traditional raiding enemy of the sedentary, agriculturalist Hausa) were given preference over foreign Hausas. The Djerma (Zarma) were also preferred to foreign Hausas.

Taking the cumulation of approximately six hundred person-responses in which foreign Hausas were a choice in a pair (100 respondents × 6 paired questions − [nonresponses]), the overall trend is clear: cocitizens of different ethnicity are preferred over coethnics of neighboring but foreign nationality (see Table 3). Had the mitigating cases of the non-Muslim Kiristas and the Maguzawa not been included (which they deliberately were, to control for religious vis-à-vis ethnic affinities), the results would have been even more strongly in the direction of national, as opposed to ethnic, consciousness. As it is, in almost two-thirds of all cases (64.4%), national preferences prevailed over ethnic ones.

Conclusion

The survey results indicate that Hausa villagers on the frontier (1) generally do not place their ethnic identity as Hausas above their national one as citizens of Nigeria or Niger and (2) express greater affinity for non-Hausa cocitizens than foreign Hausas. These results lend empirical support to the observation by Nicolas that "belonging to a modern state—Niger, Nigeria . . . —plays a more important role than the legacies of ancient states or ethnicity" (1975, 426).

Use of the term *borderline* to mean "marginal" reflects an unfortunate linguistic transformation that has carried over into social science. Frontier communities should not be ignored or overlooked in broad-based survey research because they are "peripheral," or "fringe," cases or "on the outskirts." To the contrary, much can be learned if research is not restricted by these artificially imposed limits and demarcations, and cross-border studies are conducted with all the rigor political science can muster. Such cross-border research and analysis may, in fact, prove to be the cutting edge of comparative research. Given the prominent role of Islam in the communities under consideration here, it would be particularly valuable to undertake parallel studies both in non-Muslim areas of Africa and in other non-African plural societies with significant Muslim populations (such as are found in the Indian subcontinent).

Our survey of Hausa villages in Nigeria and Niger is but an illustration of this approach. It exposes a higher-than-expected level of national consciousness among African villagers on the geographic periphery of the state, even to the apparent detriment of ethnic solidarity. It also indicates some fascinating variations in the intensity of the phenomenon, attributable to national affiliation. Above all, it serves as an example, we hope, for the kind of systematic transborder research that may shed further light on the processes of state penetration and national integration in developing countries.

Notes

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1. In a more recent use of the conventional wisdom theme, Niemi and Barkan (1987, 587), comparing age and voting turnouts in Kenya and Turkey, write, "In developed countries, a curvilinear relationship between age and turnout represents conventional wisdom; two new cases would not add to existing knowledge."

2. Of course, partitioned ethnic groups are not solely African or even Third World phenomena but are found in plural and multiethnic societies in the West as well. Lancaster's (1987) investigation of "peripheral nationalism" among the Basques of France and Spain looks at this tug between ethnic and state loyalties in a European context. Lancaster's surveys reveal (as does ours) differing levels of ethnic consciousness on the basis of citizenship (greater among Spanish than French Basques) along with an overall acceptance of inherited national (i.e., state) affiliation and boundaries.

3. The Hausa, numbering 30 million, are perhaps the largest ethnic group in all Africa. They live principally in Northern Nigeria and Niger, but may be found anywhere from Togo to the Sudan. Oil-rich Nigeria, a former British colony with a population of about 100 million, is divided into over two hundred ethnic groups (the principal ones being Hausa, Yoruba, and Igbo). Land-locked Niger, a former French colony mostly in the Sahara desert, has only 5 million persons. Besides the Hausa, there are about half a dozen other ethnic groups in Niger (most important of which are Fulani, Djerma, and Tuareg).

4. Since the sample, methodology, and analysis of the 1986 and 1983 surveys are not sufficiently comparable, there shall be no attempt to contrast them statistically.

5. Yardaji was part of Kaduna until the new state of Katsina (containing Yardaji) was carved out of Kaduna's northeast panhandle in 1988.

6. This finds institutional expression in Nigeria's federal structure, in contrast to Niger's administration as a unitary state.

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CONTOURS OF THE MODERN CONGRESSIONAL CAREER

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In the last 40 years careerism has hit Congress full force, yet we do not know much about the pros and cons of lengthy congressional careers. I describe the changes typically occurring over the course of extended stays in Congress and then show that these contours have shifted over the decades such that for most aspects of congressional service (electoral support, formal positions held, and constituency attention) there is less differentiation between a member's early and late career stages than there used to be. However, a major exception exists to the diminution of life cycle effects in Congress: pure legislative involvement (raw levels of activity combined with the extent to which a member's legislative agenda is focused and efficient), despite widely held beliefs about the death of apprenticeship, is now more than ever dependent on tenure. In relative terms, junior members are less *legislatively involved than they were a few decades ago. While many portions of congressional service are now routinized from the beginning, legislative involvement comes not just with a subcommittee chair but only with experience. This finding has clear implications both for proposals to limit congressional terms and for perceptions of congressional careerism in general.*

Political institutions shape political systems, and political careers shape political institutions. A major policymaking body and the system in which it operates cannot avoid being altered when the mean length of service in that body increases by 100%, as happened in the U.S. House of Representatives between 1860 and 1960. Modern members are staying and staying. Yet because of conceptual and methodological problems, extant research on congressional careers does not provide observers with much insight into the pros and cons of congressional careerism. I shall report the results of a study designed to provide the empirical evidence necessary to determine some of the implications of the modern congressional career.

What We Know and What We Don't

Congressional careers offer a useful window to an understanding of Congress and the behavior of its members. The study of careers can help us to comprehend electoral, institutional, and policy change. As a result, scholars have approached the topic from a variety of angles. They have analyzed the common pre-Congress and post-Congress occupations and positions (Bogue et al. 1976; Canon 1990; Eulau and Sprague 1964; Schlesinger 1966; Hain 1974; Matthews 1954). They have studied the reasons for coming to, and departing from, Congress (Cooper and West 1981a, 1981b; Fowler and McClure 1989; Frantzich 1978a, 1978b; Hibbing 1982a, 1982b; Kazee 1980; Kernell 1977; Maisel 1982). And they have studied the changing length of congressional careers (Brookshire and Duncan 1983; Bullock 1972; Casstevens and Casstevens 1981; Fiorina, Rohde, and Wissel 1975; Huntington 1965; Polsby 1968; Price 1971, 1975, 1977).

Unfortunately, the benefits of career analysis have not been fully realized. Previous research has been most concerned with the location of congressional service in typical U.S. political careers—what could be called the *external* congressional career. Work on congressional comings and goings is worthwhile and should continue, but we know far too little about the evolution of legislators *while they are in Congress.* Though it is important to understand why legislators arrive and depart, the fact is that at any given time in the modern Congress, most of them stay. These are the individuals about whom we need to know more if we are to come to grips with the modern congressional career.

Even when it has addressed internal rather than external congressional careers, previous research has been characterized by a great deal of anecdotal material and (ironic as it might seem), by a predominance of cross-sectional, rather than longitudinal, research designs. The closest that most career-based research comes to being longitudinal is to include tenure as a variable in a cross-sectional analysis or to pick a single class for longitudinal inspection. Tempting as these shortcuts may be, they do not make it possible to sort out generational, life cycle, or (for that matter) period effects.

Including tenure as an independent variable in a cross-sectional analysis is of little help. It is one thing to know that in a given Congress, second-termers do more constituency service than tenth-termers. It is quite another to know whether those tenth-termers have always ignored constituency work (generational effects) or started out actively pursuing constituency service work but gradually tired of it (life cycle effects). Selecting a single class for longitudinal analysis is not the answer, since even if we know that a single class decreased constituency service work over the course of its stay in the House, there is no way to know whether the decrease was due to period effects (which influence all members regardless of career stage) or life cycle effects (which are confined to a particular career stage). Moreover, as it turns out, the selection of a single class for analysis is often made because of its atypicality rather than typicality (consider Loomis 1988 and Schneider 1989 on the class of 1974), thus reducing the potential for generalization.

Those anxious to address questions about individual, internal congressional careers, however, are able to draw on some previous research. Asher and Weisberg's study of "the tremendous continuity in congressional voting" at the individual level (1978, 423), Bullock and Loomis's (1985) systematic investigation of patterns in the acquisition of formal party and committee positions during careers, and Parker's (1986a; 1986b) analysis of the relationship between congressional tenure and travel to the home district are all helpful.

Moreover, Donald Matthews and Richard Fenno can be interpreted as offering the beginnings of a theory concerning how the pieces of the congressional career fit together (see Matthews 1960, 241–42; Fenno 1978, chap. 6). Matthews describes a congressional life cycle in which legislators, when first elected, "aggressively pursue case work" to create "a larger circle of friends." If they survive early challengers, representatives will find themselves with "additional legislative responsibilities" and associated pressure to focus more on national problems and less on district affairs. Thus, as Washington prominence is climbing to a peak, electoral support at home is weakening and may even result in "defeat suffered at the peak of careers . . . a cruel reminder of the constituents' power" (Matthews 1960, 241–42).

Fenno also sees a cyclical component to congressional careers, but his conception sounds less foreboding to senior incumbent representatives and is applied exclusively to constituency careers. Fenno simply notes that many careers consist of two stages: the first is expansionist in which (similar to Matthews' newly elected legislators) most members "cultivate a broader reelection constituency by reaching out for additional elements of support" through such activities as attending every local event imaginable. Then, at some unspecified time, most members, feeling increasingly secure and/or decreasingly energetic, become more protective and less expansionist. In other words, they "became less interested in building supportive constituencies and most concerned about keeping the electoral support already attained" (Fenno 1978, 173).

Aside from the efforts of this handful of intrepid researchers, theoretical expectations and empirical confirmations in the area of internal congressional careers are virtually terra incognita. It is time to begin exploration in earnest. My central question here concerns the changes that take place for individual members of the U.S. House of Representatives during their careers in that body. I address change in four main areas: the electoral career, the formal position career, the legislative activity career, and the constituency activity career.

But it is not enough merely to document that as their careers wear on, representatives tend to become safer electorally, more active legislatively, less active in constituency affairs, and more likely to hold quality formal positions in the body. A key question in the study of modern congressional careers is whether or not the relationship between tenure and these variables has itself changed over the decades. Has the connection between tenure and electoral success changed from what it was 40 years ago? Is it still curvilinear, as Matthews claimed? In terms of legislative contributions, do junior members still take a backseat to their more senior colleagues? Do quality formal positions now come to members sooner in their House careers, as casual observation seems to suggest? Do senior members continue to be relative laggards in terms of devoting attention to their constituents?

These questions all relate to the extent the political life cycle description applies to modern congressional careers. It seems likely that the fit will not be good. After all, Matthews's description of the political life cycle was a description of the 1950s Senate. There is no reason to believe it must apply to the late-1980s House. Most recent research seems to suggest, if only implicitly, different contours than those outlined by the life cycle view. There have been intimations of both electoral flattening across careers (see esp. Born 1979; Jacobson 1983, 45–46), due largely to the improved electoral performance of junior members in recent years, and of increasing reluctance to alter previous patterns of behavior in legislative as well as constituency-oriented affairs (see Fenno 1978, 193; Fiorina 1977, 54; Parker 1986b, 81). Much has been made of the fact that many of the variables that previously differentiated career stages (the apprenticeship norm and possession of a party or committee leadership position) are now less likely to do so. In addition, a widely disseminated, readily adopted formula for staying in office is now thrust upon even the newest of members (avoid dangerous issues and pay scrupulous attention to the people back in the district). Little room is left for career-based change. Have these contour changes actually taken place? Has the political life cycle been replaced by career stasis in Congress?

Finally, to understand modern congressional careers we need to know how the various elements are interrelated. Strange as it may seem, the expectations of Matthews's political life cycle view are that life cycle effects are only spurious. Just as age in political participation research seems to be a surrogate for such things as a lack of roots among the young and the difficulty of getting to the polls among the elderly (see Verba and Nie 1972), Matthews sees tenure as a surrogate for the electoral insecurity of junior members and the legislative responsibilities of senior members. Strate and his colleagues correctly note that the key question is whether age goes away when these other, theoretically more poignant variables are inserted (1989, 443; see also Birren and Cunningham 1985 and, on time and presidential popularity, Mueller 1973 and Kernell 1978). What about congressional careers? Does congressional tenure have any direct effect on legislative or constituency-oriented behavior once the indirect effects of tenure *through* electoral security and formal legislative positions are controlled? If not, tenure is spurious. If so, length of experience in the House matters quite apart from a member's electoral support within the constituency and positional base within the House.

The value of employing longitudinal research to address these questions is illustrated by the leverage gained on a burgeoning policy problem. Sentiment is growing in some quarters to limit the number of terms that can be served in the U.S. Congress. Seventy percent of the American public supports the concept (Hook 1990, 568), interest groups now exist for the sole purpose of establishing the congressional equivalent of the Twenty-second Amendment, Representative William Frenzel (R-MN) has introduced the proposal in each of the last few congresses, President Bush has recently lent his support, and the popular press of late has bristled with commentaries on the pros and cons of the idea. The stimulus for much of this is the extremely low levels of turnover in the last few years. Supporters of term limitations point to minuscule entering classes (only seven incumbent representatives were defeated in 1988) and make vague statements about the need for "new blood." Opponents claim turnover is not as low as all that (as of January 1990, 55% of the House membership had entered in the 1980s) and make equally vague statements about the value of experience.

In reality, the issue should not be raw turnover levels so much as the relative contributions of junior and senior members. Do senior members provide something to the process that junior members cannot? Or in this postreform, media age are all members doing the same kind of banal posturing and constituency service? With answers to these questions, the benefits of lengthy service would be known and informed positions on the merits and demerits of term limitations specifically and congressional careerism more generally could be assumed. As of now, political scientists are faced with the fact that we have little research bearing on a reform proposal that carries with it a whole set of assumptions about the value of congressional careerism.

Were congressional careers as short now as in the nineteenth century, the opportunities afforded by studying career-based change would be slim. But this is hardly the case. Barring the unforeseen, in February 1992 Jamie Whitten (D-MS) will set the record for longest continuous service in the House of Representatives at 51 years and going strong, surpassing Carl Vinson (D-GA) and Emanuel Celler (D-NY) whose 50-year House careers ended in 1965 and 1973, respectively. The fact that these extraordinarily long careers were recorded by members serving in the last 30 years is not misleading. As has been documented many times, typical House careers have been far longer in the last 50 years than they ever have been previously. Indeed, in the first few decades of Congress's existence, representatives could hardly wait to depart. Price notes that of the 242 House members in the Twenty-fourth Congress (1835–36), 128 were not in the Twenty-fifth and most departures were voluntary (1977, 38). This is a far cry from the "careerism" rampant in the modern Congress (see Bullock 1972). When members typically stay in Congress for only a few months, career-based research is difficult and basically meaningless; but when a large number of individuals are staying for decades, career-based research becomes not only possible but necessary.

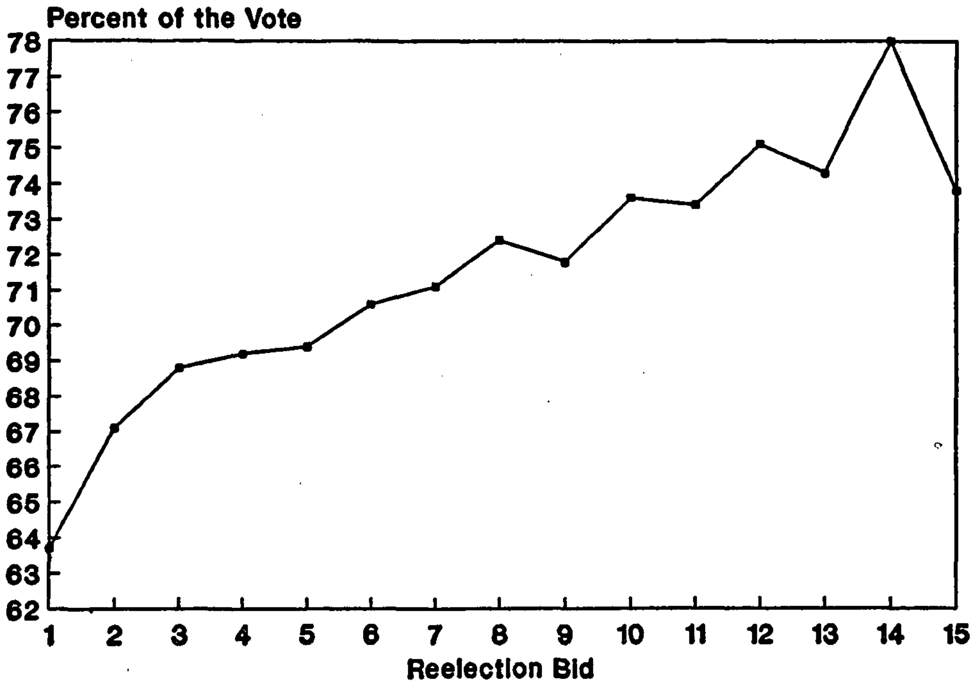
Analyzing Congressional Careers

Conducting individual-level, longitudinal analysis involves substantial data collection efforts.[1] Existing data sets must be restructured so that they run along individual careers rather than across them. Even when this is done, many issues remain. The initial goal was to obtain information on all House careers touching the 1946–86 era. Unfortunately, shorter time periods and smaller groups of representatives were sometimes all that could be used. To cite just a few examples, only Democrats serving between 1955 and 1986 were used for the portions of the analysis dealing with formal House positions (since this time and group corresponds to majority party members who thus are eligible for committee and subcommittee leadership positions). Only legislatively active representatives (introducing at least four bills) could be used for purposes of calculating legislative specialization and efficiency scores. And only post-1970 careers could be used for the portions of the analysis dealing with trips home because of data reliability problems prior to that date.

What little previous research there is on congressional careers has been plagued by an inability to standardize for an evolving institutional context. Congress has changed tremendously since 1946. The electoral environment has been transformed from one dominated by partisan swings to one dominated by candidate-centered contests (see Bauer and Hibbing 1989; Jacobson 1987). The number of formal positions has gone through the roof (see Loomis 1984; Sinclair 1983). Legislation used to take place in discrete bills, whereas now much of it occurs on three or four large omnibus packages (see Ehrenhalt 1986). Cosponsorship is now permitted; rules have changed; and a revolution has occurred in the manner representatives shower attention—albeit routinized attention—on constituents (see Fiorina 1977; Parker 1986b). If we are to detect life cycle effects, great care will need to be taken in controlling for these contextual changes.

In much of the analysis to follow, I shall control them by utilizing z-scores for each individual Congress. These standardized scores indicate the relationship of each representative's behavior or situation to the mean for all representatives in that particular Congress. In this way, traveling home 15 times in the Ninetieth Congress may be equivalent in relative terms to traveling home 25 times in the Ninety-fifth, since the mean for all representatives increased during that 10-year period (in conjunction with expanded reimbursement opportunities). While this procedure controls for period effects, care must be taken to remember that what is being discussed is *relative*, rather than absolute, activity levels. Most previous research has been unable to standardize in this fashion because the data base involved did not include all members of every Congress. When only certain classes are used, there is no way to produce a z-score on a Congress-by-Congress basis; and without standardization, it is extremely difficult to get an accurate reading of unadulterated life cycle effects.

Figure 1. Mean Share of the Vote in House Elections by Tenure, 1946–84

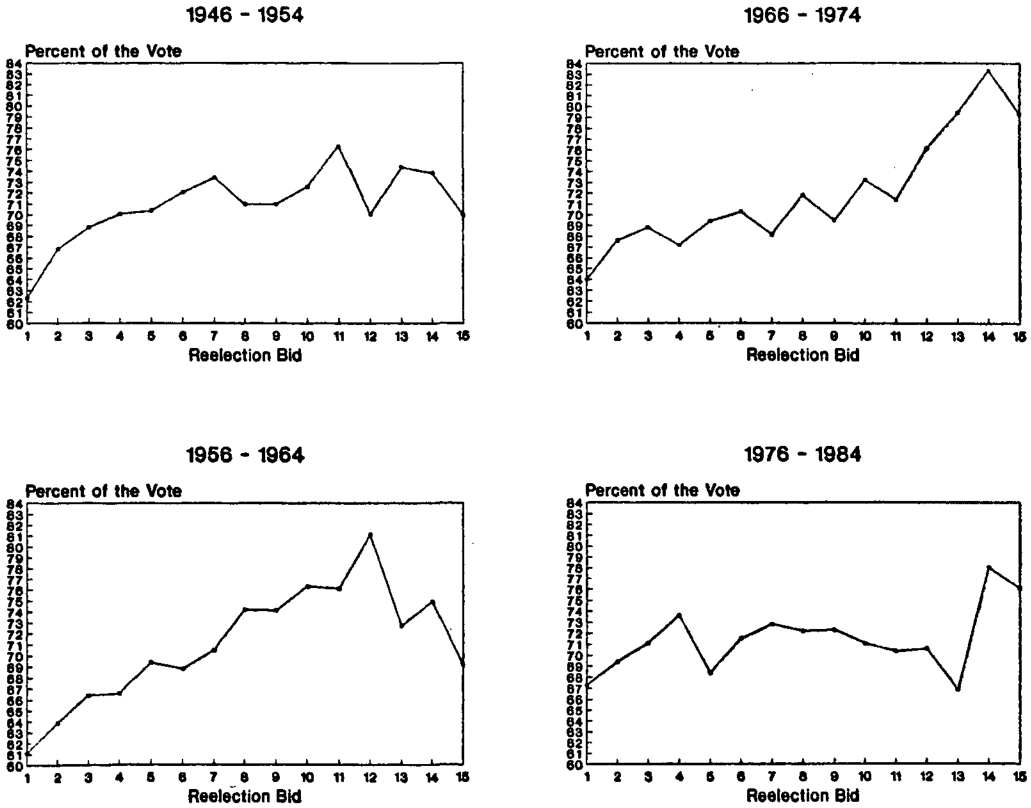


The Electoral Career

Electoral concerns have been prominently featured as the engine driving congressional activity, particularly early in the career. This view is found in Mayhew's classic portrayal of Congress (1974) as well as Matthews's description of the political life cycle. Countless studies have attempted to account for changes in the mean vote totals for incumbent representatives across congresses; but only a few works (e.g., Alford and Hibbing 1981; Born 1979; Collie 1981) have traced individuals or even classes across the years.[2] For whatever reason, aggregate change over time has been more attractive as a research topic than individual change over careers.

Does the curvilinear life cycle pattern represent the typical contour of postwar electoral careers? To some extent. As can be seen in Figure 1, portions of the life cycle view withstand systematic analysis. The pattern for mean electoral support in the post-1946 years appears slightly curvilinear across tenure categories. The most rapid increase comes prior to the fourth term at which time the share of the vote begins slow but steady improvement until about the fourteenth term. This much parallels the life cycle view. After 25 or 30 years in the House, diminishing cases produce some instability; however, we never see a clear downturn in support, as was predicted by the life cycle conceptualization. There is a drop (to 74%) from the fourteenth to the fifteenth term; but from the fifteenth to the sixteenth (not shown) support bounces way up again. The notion that very senior members are frequently vulnerable is er-

Figure 2. Decade by Decade Analysis of the Vote in House Elections by Tenure, 1946–84



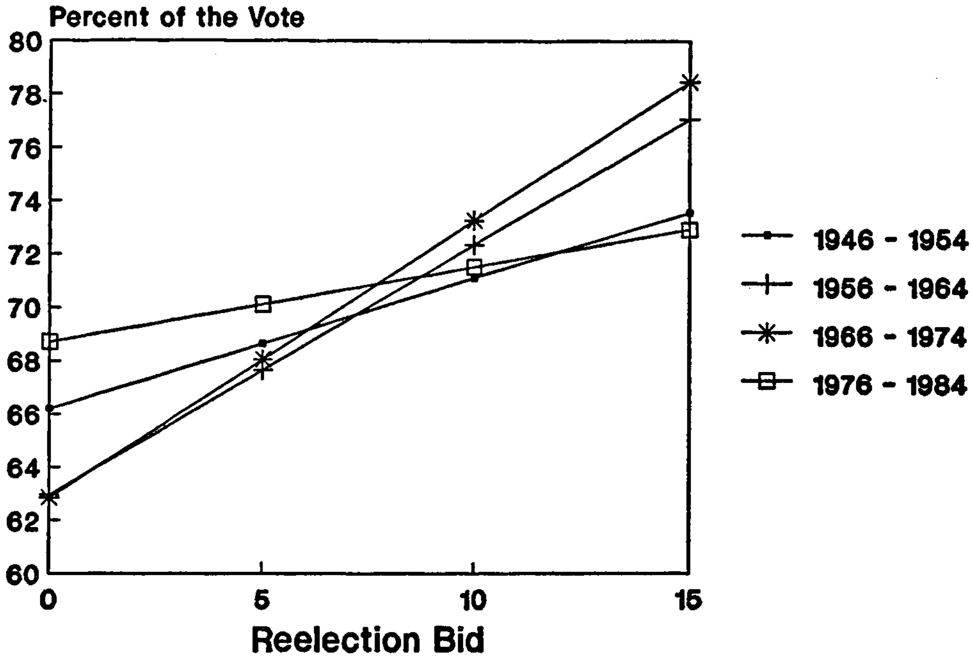
roneous, probably the product of a few highly salient instances of visible senior people like Emanuel Celler (D-NY) and Joseph Martin (R-MA) encountering late-career electoral problems.

A more intriguing issue concerns the extent to which the highly aggregated career pattern in Figure 1 has itself been reshaped in the last 40 years. As alluded to earlier, the strong performance of many recent members in their first reelection bid encourages the hypothesis that electoral careers are flatter than they once were. The best way to illustrate the manner in which electoral careers have changed contours is to break the data into four decades, although even this level of aggregation must be undertaken cautiously. The results are presented in Figure 2 in the form of four separate graphs.

The change in the contours of the House electoral career could hardly be seen more clearly. The pattern in the first two decades of the postwar period is obviously curvilinear with noticeable dips after 11 or 12 terms. But the two most recent decades yield entirely different patterns. In the 1966–74 period, there is a strongly linear pattern with big increases present even at the upper ends of the register. Most surprisingly, from the mid-1970s to the mid-1980s, incumbent vote share rises for three or four terms but then actually begins a slow decline all the way out to the fourteenth reelection bid.

Additional evidence of the change can

Figure 3. Lines of Best Fit for the Relationship between Vote Share and Tenure by Decade, 1946–84



be obtained by fitting lines to the four sets of data points in Figure 2. This is done in Figure 3. The ordinary least squares "lines of best fit" show a steadily increasing slope from decade to decade until the most recent period. Here the slope drops to its lowest (only about .3% per term); and the predicted difference between first reelection bid and fifteenth is a scant 3.5%, a far cry from the distinctive electoral performance of junior and senior members in the "old days" of the 1950s and 1960s.[3]

But in light of Figure 2, it is apparent that fitting straight lines to the relationship between tenure and vote share is a misspecification. The extent to which curvilinearity exists is indicated by Table 1 where the fit of a linear specification is compared with the fit of a curvilinear (logged independent variable) specification. As is suggested from "eyeballing" Figure 2, the relationship in the two earlier decades was clearly curvilinear with increments in later portions of the career becoming smaller and perhaps even negative. A strong linear relationship in the next decade is then followed by a weak relationship in the final decade of the analysis—a relationship that is not particularly linear or curvilinear, but mostly flat.

This, in combination with similar results for the percentage of incumbents who were victorious in reelection bids[4] leads to the conclusion that in recent years tenure level has been largely irrelevant to the electoral performance of House incumbents. In the 1950s incumbents seeking their first reelection to the House were successful about 80% of the time and averaged approximately 60%–65% of the

Table 1. Selected Summary Statistics of the Relationship between Tenure and Share of the Vote Received

Decade	Linear Adjusted R^2	Logged Adjusted R^2	Curvilinear R^2 − Linear R^2
1946–54	.40	.65	.25
1956–64	.54	.68	.14
1966–74	.80	.62	–.18
1976–84	.14	.14	.00

vote. After a few more terms, they were successful nearly 95% of the time and received around 70% of the vote. Such variance across tenure levels is not common anymore. In the late 1970s and early 1980s incumbents seeking their first reelection to the House were successful approximately 93% of the time and averaged 68% of the two-party vote. But after a few terms modern representatives seem to be in virtually the same spot, with reelection rates around 93% and share of the vote averaging still less than 70%. If the behavior of modern representatives is cyclical, it is not because of career-based changes in electoral performance.[5]

The Formal Position Career

The phrase *internal House career* probably brings to mind the progression of members through the maze of party and committee positions. For most members, these positions provide an identity and would appear to shape the nature and extent of legislative and constituency activities. (Remember the inverse relationship between legislative involvement and constituency attention posited by the life cycle view.)

The overall relationship between tenure and the possession of quality formal positions is so obvious as to be uninteresting. No one doubts that because of general institutional tendencies toward preferring experienced leaders and because of specific House features such as the existence of a seniority norm in the selection of committee chairs, tenure and the possession of formal positions will be positively related. This is exactly what the overall figures demonstrate. What is of more interest, however, are potential changes in the rate and timing of position acquisition; and this is where confusion exists. The conventional wisdom that positions are now acquired earlier in the House career has not received strong empirical support. In the best analysis available, Bullock and Loomis find that members of more recent classes often appear to be slower to advance to certain positions than do members of earlier classes (1985, 75–76; see also Loomis 1984), thereby directly contradicting traditional expectations. At other times, their findings support conventional wisdom. In light of these inconsistent results, further investigation with more complete data seems warranted.

Bullock and Loomis present separate findings for many different positions within the House; as a consequence, their results are predictably confusing. In order to place all House positions on a single metric, I invoked the assistance of close congressional observers, including top-level congressional aides, journalists on the Capitol Hill beat, and political scientists specializing in legislative politics. In a mail survey, I asked these individuals to place an array of House positions on a scale between 1 and 100, with 100 being arbitrarily assigned to the position of Speaker of the House.[6] Table 2 reports the

Table 2. Rating of Selected House Positions in the Modern Era

Position	Rating of Position
Party leadership	
Speaker	100
Majority leader	80
Majority whip	59
Deputy whip	34
Caucus chair	39
Member of Steering and Policy Committee	30
Committee leadership	
Chair of prestige committee	72
Chair of policy-constituency committee	59
Chair of less-desirable committee	34
Subcommittee leadership	
Chair of appropriations subcommittee	56
Chair of policy-constituency subcommittee	40
Chair of less-desirable subcommittee	17
Committee membership	
Member of prestige committee	34
Member of policy-constituency committee	17
Member of less-desirable committee	1

mean ratings respondents gave to the selected positions.

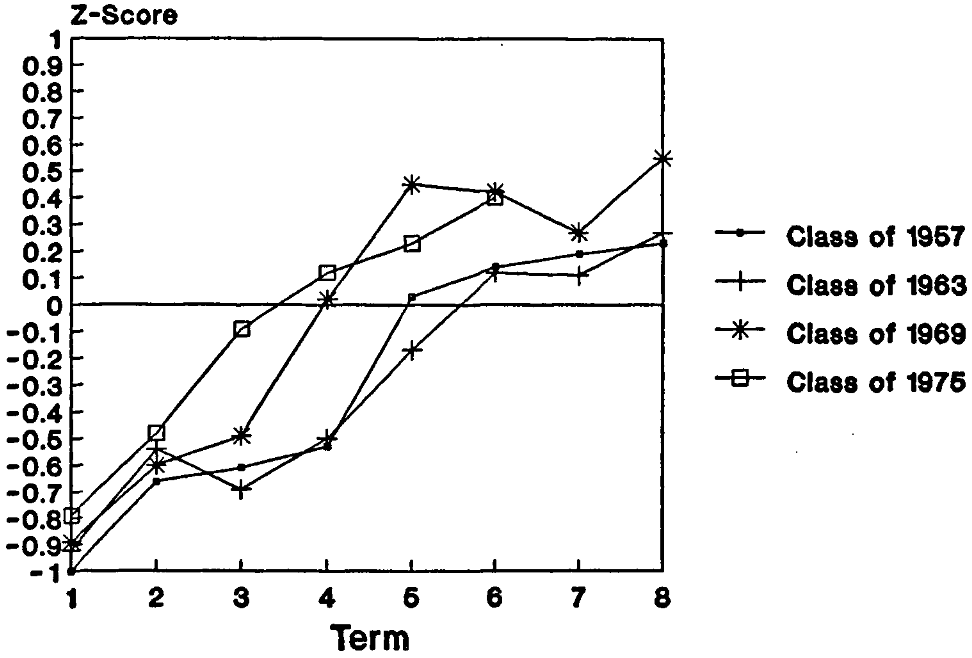
On the basis of these mean figures, numerical values can be assigned to the specific position or positions held by each majority party member in each Congress from 1955 to 1986. For example, in the Eighty-fourth Congress, James Haley (D-FL) was chairman of the Indian Affairs Subcommittee of the Interior Committee. Since Interior is a constituency committee, and since Haley was also a member of the Veterans Affairs Committee, his score for the 84th Congress would be 57 (40 + 17). This process was repeated for each term served by each Democratic member.

Of course, increases in the number of subcommittees, in the size of committees, and in the number of party positions all cause mean scores to be much higher in recent Congresses than in earlier. In the Eighty-eighth Congress (1963–64) the mean for all Democrats was 37.9. By the Ninety-seventh Congress (1981–82), it was 50.6. To facilitate comparisons of the different classes, I have computed the z-score for each representative. Mean z-scores for the various classes at each stage of their careers should allow us to draw conclusions about the share of formal positions held by a given class relative to all formal positions available in the pertinent congress.

Are junior members obtaining a greater share of the positions available, or are suspicions along these lines merely the products of growth in the number of positions? Figure 4 presents the contours of position acquisition for four evenly spaced classes. Though data are available on all classes since 1955, presenting more classes than these four generates unnecessary visual clutter and, more importantly, does not affect general conclusions. Even when the growth in the number of positions is taken into consideration (by standardization), junior members now hold a greater share of these positions than was the case in most earlier careers. The classes of 1975 and 1969 rise faster than the classes of 1963 and 1957. Further analysis of all available classes indicates that the major change in contour seems to have come between 1967 and 1971.

While the contour shift described in conventional wisdom is apparent in these data, some qualifications are in order. First, the shift is more confined than may have been thought, applying primarily to only the third, fourth, and fifth terms of congressional service. From the first to the second term, members acquire a slightly greater share of the positions; and both early and recent second-termers are typically about half a standard deviation below the mean for all members in the

Figure 4. Standardized Mean Position Rating for House Democratic Classes of 1957, 1963, 1969, and 1975



pertinent Congress. But in the third term, the distinctive features of the post-1969 position career are apparent. Third-termers in more recent Congresses have a much greater share of the action than third-termers in pre-1969 Congresses. The quicker acquisition of quality formal position continues in the fourth and fifth terms. On average, fourth-termers in post-1971 Congresses rank above the mean position score for their colleagues (z-score of .11). Fourth-termers in earlier Congresses, however, rank one-third of a standard deviation below the mean for whatever congress marked their fourth term (z-score of −.33). By the fifth term, the typical old-style career has yet to reach the mean position rating while the new-style career continues its climb.

But between the fifth and sixth terms, the old career narrows the gap substantially. By the seventh and eighth terms, there is in essence no difference between the old- and new-style career. As suggested by conventional wisdom, the new style of position career is one in which members acquire positions more quickly. In the modern House, members improve their formal position situation markedly between their fourth and tenth years of service. Many move into subcommittee chairmanships, and most do even better than this. In the House of the 1960s and late 1950s, the major strides came after at least eight years of service. Not until then did members begin to acquire key committee, subcommittee, and party positions.

Legislative responsibilities (at least in the sense of holding legislatively important positions) seem to come sooner in the modern career than was implied by the political life cycle view of Congress. But what about the actual activities involved with legislating: introducing bills, amending bills, making speeches, formulating a legislative agenda, and shepherding bills through the various legislative stages. How do these behaviors change with the progression of a career, and are the contours of these legislative activity careers different now than they used to be?

The Legislative Activity Career

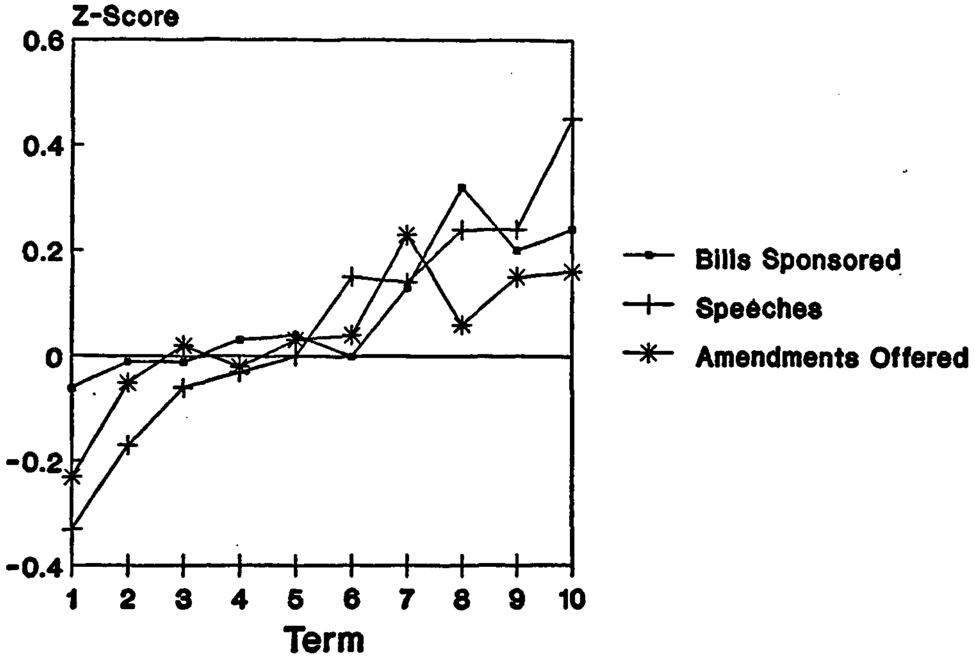
Early in the summer of 1988, a former representative attempted to communicate to me the seriousness with which the apprenticeship norm used to be taken in the House. He said, "When I came to Congress in 1960, Carl Vinson [D-GA and long-time chair of the Armed Services Committee] had a rule that first-term committee members were permitted one question in all the deliberations that took place during *that term; that's it*—one question in an entire term; they were permitted two questions in their second term; three in their third; and so on." Another former member told me that the major changes in his legislative activity were produced less by what he was permitted to do than by his understanding of what was possible and what was not. He said, "Of course my involvement in legislation changed over my career. When I was young I offered an amendment to a bill we were considering in the Interior Committee. Well, all this amendment did was make [committee chair and dominant force Wayne] Aspinall mad as hell. The amendment was adopted, but this in turn doomed the bill. I would never have done that years later." These comments encourage more systematic analysis of the legislative differences between junior and senior members, and they also lead to questions about whether or not any differences that are discovered are as severe now as they were when the first representative quoted entered the House in 1960. To address these questions, I first computed five measures of legislative activity.

The first is simply an index of floor speaking, counted as the number of noncontiguous page references to statements made by a member as recorded in the *Congressional Record.* Insertions and speeches not related to actual legislation were excluded. The second measure is the number of amendments offered by a member during floor debate. This information was also collected from the annual index of the *Congressional Record.* The third measure is the number of bills sponsored by a member. By using the *Congressional Index* rather than the *Congressional Record,* it is possible to separate real sponsors from the hordes of cosponsors that frequently descend upon a bill in the modern legislative environment.

The fourth and fifth measures are different in that they attempt to tap more than raw activity levels. The fourth is legislative specialization and is computed by dividing the total number of a representative's bills into the number of those bills that were referred to the most frequently involved committee. To take an example, suppose Representative Smith sponsored 10 pieces of legislation and that 4 were referred to Interior, 5 to Agriculture, and 1 to Merchant Marine. The specialization score would be 50 since 50% of Smith's legislation went to a single committee. The final measure is legislative efficiency and has two components. The first is simply the percentage of a member's bills passing the House during that particular Congress. The second component is the percentage of a member's bills that are reported out of committee. These two components are equally weighted and combined into a single measure. As before, private bills, cosponsored bills, and all resolutions are ignored.

For purposes of presentation, it makes sense to treat the raw activity measures separately from the specialization and efficiency indices. Turning first to raw activity, Figure 5 indicates that senior members

Figure 5. Standardized Measures of Legislative Activity by Tenure, 1953–85

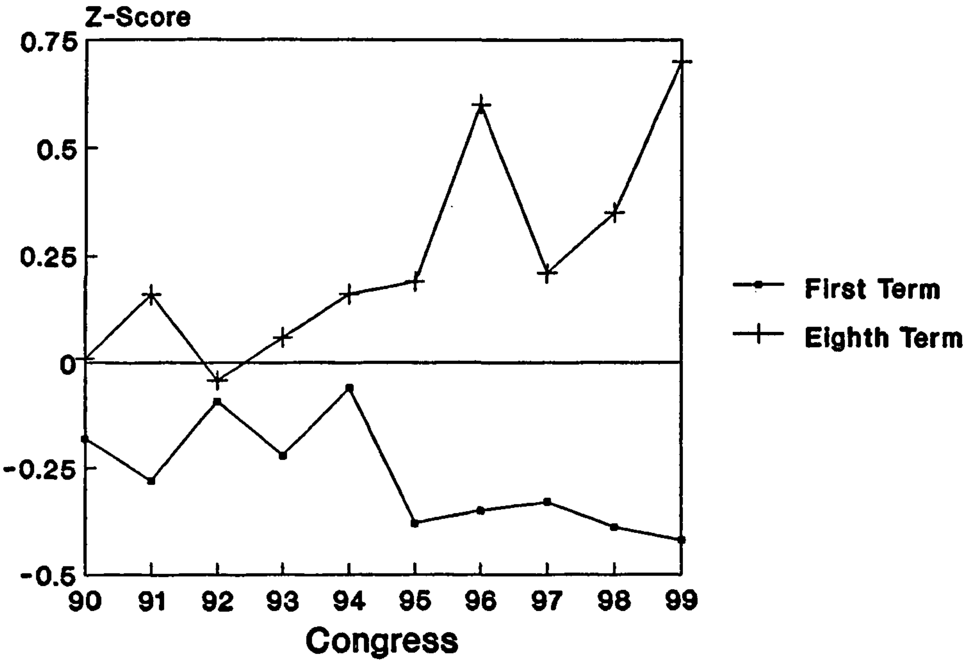


are indeed more active than junior members, at least in terms of giving speeches and offering bills and amendments. This relationship between tenure and legislative activity withstands standardization for the sizable changes in bill introduction (see Ornstein, Mann, and Malbin 1990) and in floor emendation activity (see Smith 1986). This much is unsurprising.

But some most surprising changes have taken place over the years in the degree to which junior members are less active than senior members. Much has been written about the demise of the apprenticeship norm (see Dodd and Oppenheimer 1981; Rohde 1988; and Schneier 1988—just to name a few). Unlike the situation facing the representative quoted above when he entered the House in 1960, modern representatives, it is thought, are free to participate from the minute they enter Congress. Figure 6 suggests that whatever the status of apprenticeship as a norm, in terms of actual behavior on the House floor, recently elected members are actually less active in relative terms than they were 20 years ago. In this figure, speech making, amendment offering, and bill sponsorship have been combined into a single index of legislative activity. Whereas in the Ninetieth to Ninety-fourth Congresses, first-termers were only a little less active than eighth-termers, from the Ninety-fifth through the Ninety-ninth Congresses, first-termers were significantly less active than eighth-termers.

But legislative involvement is not adequately captured merely by counting speeches, amendments, and bills. Tapping more subjective elements of legislative behavior, such as specialization and efficiency, is difficult but worth the effort. By employing the measures described earlier, it is possible to determine whether or not

Figure 6. Relative Legislative Activity Levels of First-Term and Eighth-Term Representatives, 1967–86



representatives usually become more focused and efficient as experience grows. As can be seen in Figure 7, these measures typically increase markedly with age, making especially rapid gains after the seventh or eighth terms. With regard to specialization, the findings suggest the shift from expansionist to protectionist strategies detected by Fenno may apply to more than just the constituency career. As the years wear on, the typical representative develops a more focused (and usually more successful) legislative agenda. There is a detectable decrease in legislative breadth.

Even more noteworthy perhaps is the fact that contrary to other areas, where we have seen a flattening in the contours of careers, the over-career increases in specialization and efficiency are as large today as they were 20 years ago. In Figure 8, four evenly spaced classes belie no tendency for recent classes to be flatter than earlier classes with regard to legislative specialization. The same conclusion is derived when other classes are analyzed as well as when legislative efficiency is substituted for legislative specialization. The contours of many aspects of congressional careers have changed so that there is less reason to differentiate between junior and senior members, but this is not true of legislative activities and agendas. Senior members are the most active, the most specialized, and the most efficient legislators (in a simple-minded batting average sense); this finding is in some ways consistent with Dodd's (1983) speculation concerning legislative "mastery." And the gap between senior and junior members in this area has, if anything, widened in recent years.

Figure 7. Standardized Legislative Efficiency and Specialization by Tenure, 1953–85

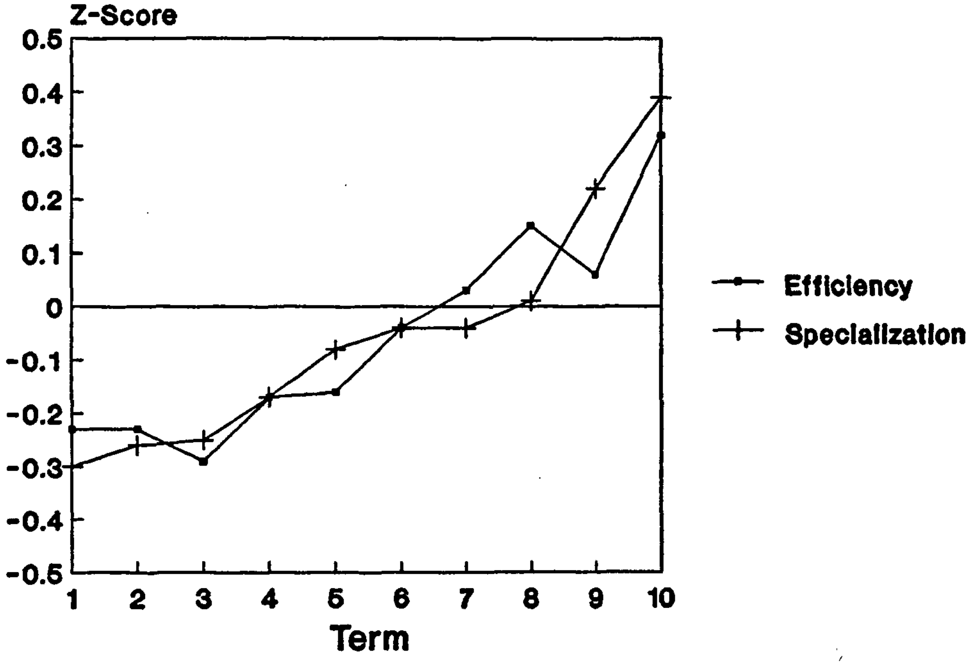


Figure 8. Class-by-Class Analysis of the Relationship between Specialization and Tenure, Classes of 1957, 1961, 1967, and 1971

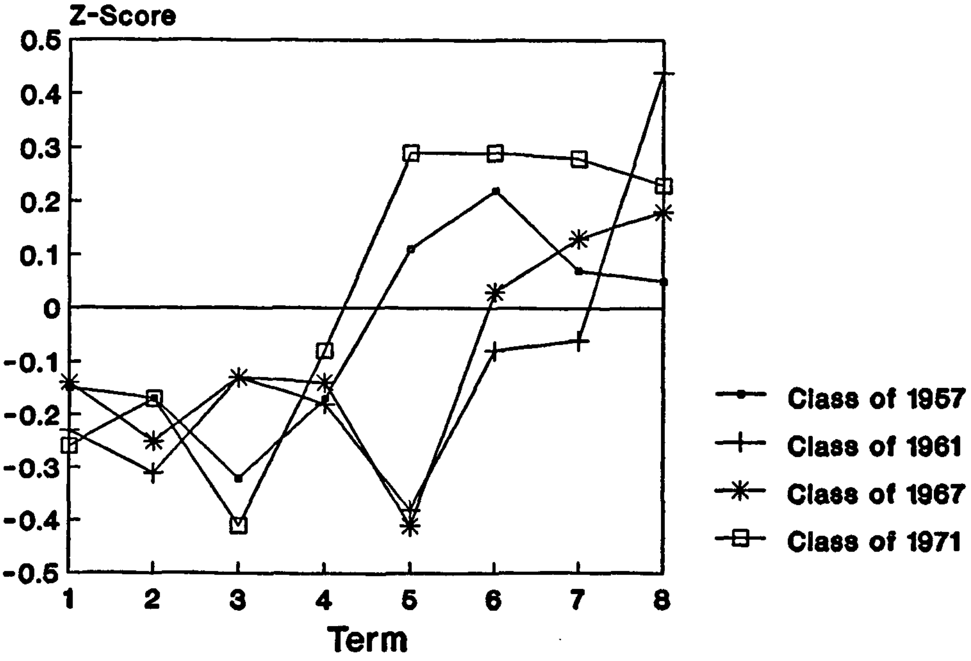
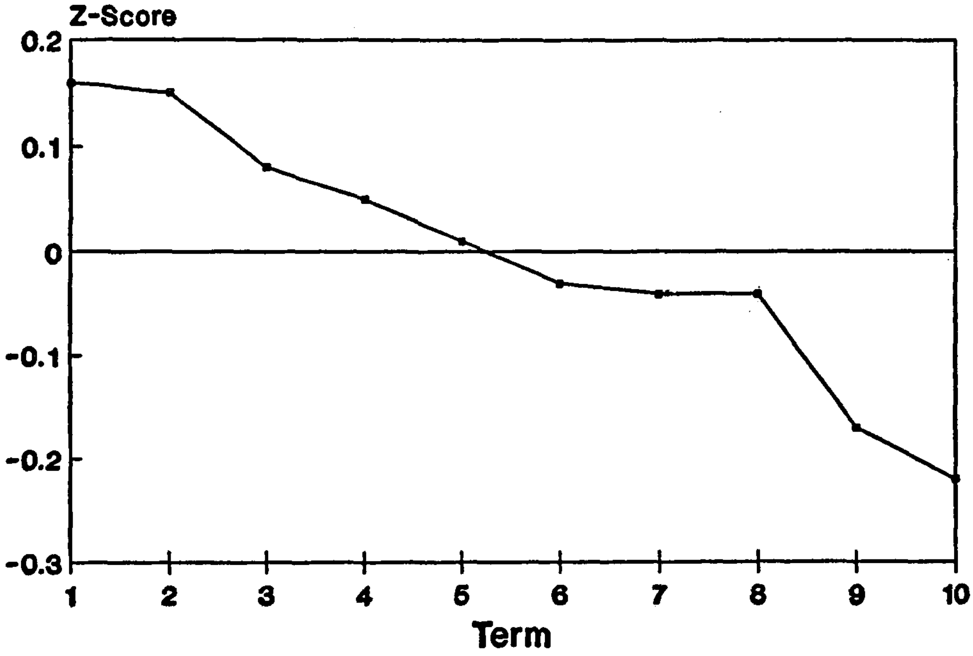


Figure 9. Standardized Travel Back to the District in the First Year of Each Congress by Tenure, 1971–85

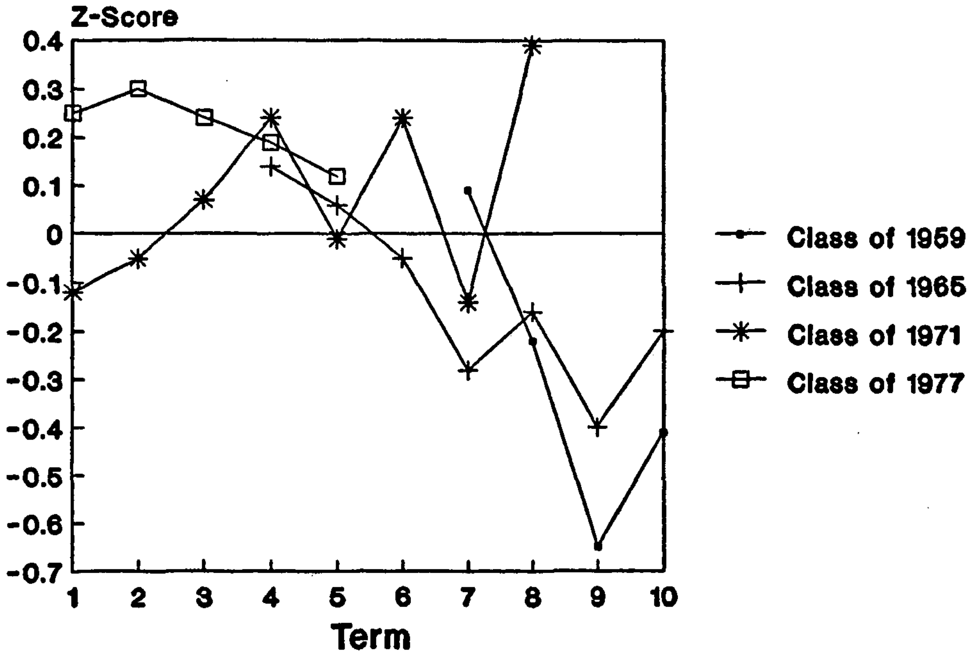


The Constituency Activity Career

But legislating is only a part of the life of members of Congress; they also represent diverse constituencies, and actions within these constituencies can be even more time-consuming than actions back in Washington. Congressional scholars were slow to appreciate this situation; but in recent years, spurred largely by the work of Richard Fenno (1978), much attention has been given to constituency-oriented activities.

One of the most basic elements of such activity is simply traveling back to the district. The *Report of the Clerk of the House* contains information on the number of trips home for which members sought reimbursement. This figure is likely to correlate reasonably well with actual trips home *except* for representatives whose districts are very close to Washington.[7] As with many of the other measures utilized in this article, substantial change in mean activity levels has taken place. The mean number of reimbursed trips home in the first year of each Congress increased from about 9 in the Ninety-second Congress to 27 in the Ninety-sixth. Since then, however, travel home has decreased back to around 20 trips in the first year of the Ninety-ninth. These changes are primarily attributable to alterations in the number of trips for which reimbursement was permitted. This maximum increased from 12 in 1970 to 33 in 1977, after which the limit was taken off and consolidated office expenses were established in which travel home would diminish money available for other office needs. Reimbursed travel home responded predictably by decreasing.

Figure 10. Class-by-Class Analysis of the Relationship between Travel Back to the District and Tenure, Classes of 1959, 1965, 1971, and 1977



In light of these aggretate changes, standardization is again necessary if we are to assess life cycle effects. The small number of trips home in the 1960s encourages a more limited time period than has been employed elsewhere in this study. Even so, the typical pattern in the 1970s and 1980s has been one in which, as expected, relative levels of travel to the home district diminish with tenure. First-termers travel home more than the mean, while from about the sixth term on, members typically are below the overall mean (see Figure 9).

The restricted time period makes it very difficult to draw conclusions about any change in the contours of travel home. In Figure 10 we find the patterns for portions of the careers of four classes. There is some suggestion that the dropoff in relative travel across careers is sharper for the class of 1959 and the class of 1964. The two more recent classes (1971 and 1977) maintain their travel home or perhaps even increase it. But to be honest, if declines are focused in the latter portions of careers, nothing conclusive can be said until the classes of 1971 and 1977 have been in the House for a longer period of time. The best that can be done here is to point out that preliminary indications are consistent with notions that modern representatives are doing a better job of maintaining (in relative terms) their early pace of traveling home than was the case with slightly earlier careers.[8]

Assigning staff members to offices back in the district rather than in Washington is another commonly used measure of attention to the home district (see Shiff and Smith 1983; Cain, Ferejohn, and Fiorina n.d.). Utilizing the *Congressional Staff*

Directory I recorded the percentage of staffers assigned back to the district. To conserve space, results will not be presented graphically; but even after controlling for the overall increase in district staff presence (to nearly 50% of all staffers in recent years), senior members are relatively less likely to send staffers to the district than are junior members (the data base runs from 1959 to 1986). As was the case with travel home, there is some indication that senior representatives recently have not, in relative terms, brought staffers back to Washington as readily as they did several Congresses ago. Especially since 1973, career-based change in staff assignments appears to have been flatter than it used to be.

So both measures of constituency attention employed here indicate that senior members are less likely to give attention to constituents than junior members. At the same time, it appears as though the typical pattern in very recent careers is for senior members to maintain relative attention levels. The drop in attention is now more muted than it used to be. In this respect, the political life cycle view of congressional careers is less applicable than it was 30 years ago.

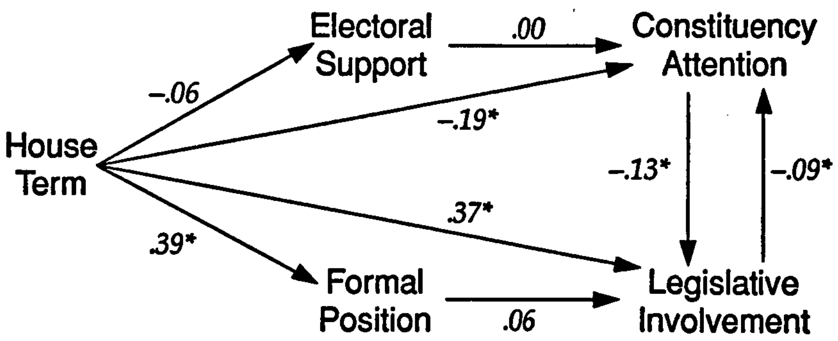
The Integrated Congressional Career

Now that some sense of the bivariate relationships between tenure and these various measures of behavior has been provided, efforts must be made to determine whether tenure is the real cause of these changes or whether the effects of tenure are actually spurious. In this sense, attention is being turned from such matters as *whether* senior members pay less attention to constituents to *why* senior members pay less attention to constituents. Is it simply because of their career stage or is it because of increasing electoral safety and heightened legislative involvement, as posited by the political life cycle view? Relatedly, do senior members become more legislatively active and specialized because of their experience in the ways of the legislative process or because when they are senior, they are more likely to possess quality, legislatively relevant formal positions?

To answer these important questions, variables must be brought together in a single model (see Figure 11). There are eight arrows: four of them (the ones emanating from House Term) have been addressed previously. However, the arrows that do not begin at the extreme left of the figure have not been discussed, and many of these remaining arrows represent vital relationships.[9] What patterns are anticipated? Most controversially, perhaps, I expect tenure to exert an independent influence on legislative and constituency-oriented activity even when electoral support and formal positions are controlled. Estimating this model should demonstrate whether increasing tenure matters on its own or simply because it tends to correlate with these intermediate variables. Will the effects of tenure fade away with the introduction of "more substantive" variables like legislative positions and electoral support in the same way that the effects of age on political participation wash out when "roots," education, and mobility are controlled? I think not. Congressional experience is different from aging and quite possibly will continue to have direct effects on behavior even with appropriate controls.

Determining whether or not the relationships anticipated are indeed present requires several procedural and methodological alterations. First, the analysis was limited to members of the House who entered between 1955 and 1973 and who served at least eight terms. Two-hundred-and-five individuals met these restrictions. Unfortunately, because the data on trips home extend only back to 1970 and because the data on formal positions ap-

Figure 11. Estimating the Integrated Model with Two-Stage Least Squares



n = 448.
*$p \leq .05$.

ply only to Democrats, a smaller number will actually be eligible for inclusion. Degrees of freedom are not a problem since the unit of analysis will be individual Congresses for each particular member. Even so, readers should remember that the estimates presented in Figure 11 are based on Democrats serving from 1970 on.

The model would be easier to estimate were it not for the nonrecursive qualities introduced by the double arrow between constituency attention and legislative involvement. At all other junctures, the arrows move from left to right. Since it is theoretically sensible to hypothesize that constituency attention inversely affects legislative involvement and that legislative involvement inversely affects constituency attention, an estimation bias would result if each were used to predict the other or if the two arrows were omitted. Consequently, I employed two-stage rather than ordinary least squares. Thus, the link from legislative involvement to constituency attention is really the link from predicted legislative involvement to actual constituency attention and the link from constituency attention to legislative involvement is really the link from *predicted* constituency attention to actual legislative involvement.

In Figure 11 two-stage least squares standardized regression coefficients are presented along with an indication of whether or not they achieve statistical significance at the .05 level (one-tailed test). It would appear from the figure that expectations are generally supported. There is a strong positive relationship between increasing tenure in the House and the formal positions a member holds. There is not, however, a corresponding relationship between increasing tenure and electoral support. This is less surprising given the fact that the complete model can only be estimated for post-1970 representatives and given our earlier discovery that tenure and electoral support are no longer related (see Figure 3).

The intermediate variables of formal position and electoral support have very little effect on the behavioral variables of legislative involvement and constituency attention once other variables in the equation are held constant. Electoral support in this formulation does not have an independent influence on constituency attention, and formal position has little effect on legislative activity. So the indirect links of tenure *through* electoral support and formal position are also weak. As expected, there is a tradeoff between legislative and constituency-oriented behaviors. Attention to one appears to diminish attention to the other, ceteris paribus.

What comes through most clearly are the direct relationships between increasing tenure and the behavioral variables. The heightened legislative involvement characteristic of later career stages is not attributable to the improved formal positions that usually come with extended tenure. Rather, legislative involvement is primarily due to having been in the House for many years. Similarly, diminished levels of constituency attention derive directly from extended tenure rather than indirectly through improved electoral support. On the whole, these results make it difficult to deny that increasing House tenure has an important independent effect on legislative and constituency activity—an effect that for too long has gone unappreciated.

As James Stimson notes in his discussion of pooled time series data, standard least squares techniques "simply ignore the pooled structure of the data . . . each case is treated as independent of all others, not as part of a set of related observations" (1985, 918). Consequently, autocorrelation and heteroscedasticity will likely be more serious problems unless between-unit variation is quite small relative to over-time variation. In our case, the "units" are the individual representatives, and time is the multiple terms for which we have data on representatives. Needless to say, there is substantial across-representative variation.

The most intuitive of the three solutions Stimson proposes for this problematic situation is to include dummy variables to pick up this between-unit variation. When this is done, the results are fairly similar to those presented in Figure 11, with a couple of exceptions. The biggest change involves the relationship between tenure and constituency attention. With the dummies included, the coefficient remains negative but drops below traditionally accepted standards of statistical significance. This means that attempts to understand variation in constituency attention are not particularly successful, since all the other path coefficients on the top half of the figure continue to be small. The strong relationship between tenure and legislative activity is actually improved by the inclusion of the dummy variables (beta = .48). As before, the connection between tenure and formal position is strong (beta = .51), but that between position and legislative involvement is weak (beta = .02).

Conclusion

What can be learned from a longitudinal approach to congressional activities? Over the course of a typical postwar congressional career electoral support improves; positions within the institution expand or improve; legislative activity, specialization, and efficiency increase; and attention to district affairs diminishes in relative terms.[10] Perhaps more importantly, the findings show that this "typical" career conceals significant evolution in the nature of congressional careers. Specifically, representatives used to do much better electorally as senior members than when they were junior members, but now electoral performance is quite similar regardless of tenure level. Senior members used to acquire quality formal positions only after several terms of service, but now these positions are frequently obtained early in a career. Senior members used to do substantially less constituency service work than junior members, but it now appears as though these differences are slight. In all these respects, the distinctiveness of particular career stages is much less than it used to be.

As such, the modern congressional career appears more static than cyclical. This much is consistent with impressionistic knowledge of changes in U.S. politics during the last 40 years. We know, for example, that newly elected members of Congress are today presented with a battery of seminars, workshops, retreats, lectures, and publications, all designed to teach them how to organize a congressional office, how to utilize congressional procedures to generate publicity, and (more generally) how to get reelected. We know that advisors, pollsters, interest groups, and political parties are there with advice about the district and the process. We know that there is a more egalitarian ethos in Congress than existed 30 years ago and that there are more opportunities for junior members than there used to be. We know that representatives are watched more carefully than they have ever been and that inconsistency may now create real problems. The motivation for staying with the previous formula is stronger than ever in Congress even though constituents are not usually the direct source of the scrutiny. Every contribution received, every junket taken, every vote missed, every roll call scale released, every piece of franked mail sent, every joke told, every staffer fired, every bill introduced, and every trip not taken home all now have the potential to be major issues in the next campaign, or the next, or the next. Why venture off the well-trod path in such a climate?

As a result, it seems that members are constantly working at putting themselves "on display" and that little time is left for real legislative growth. On the basis of these data, it would be easy to conclude that the modern House sorely misses the old-style career's mandatory period of anonymous immersion in institutional mores and policy-oriented factual research—a career stage that was an absolute prerequisite for the "coming out" of a fully flowered, honest-to-goodness senior legislator. It would be easy to conclude that modern careers are instead characterized only by more of the same inane grandstanding, pontificating, and gamesmanship, year after year, term after term, decade after decade. Vote the same as you have in the past, ignore the issues as much as possible, grab a position of authority early and milk it for as much publicity as possible, and hope for the best in an idiosyncratic electoral environment.

But the data analyzed here demonstrate that this is only part of the story. A glaring exception exists to the conclusion that congressional careers are more static than they used to be. Actual involvement in legislative matters (in terms of raw activity levels, promoting a focused legislative agenda, and shepherding bills through the legislative process) is more strongly related to tenure than ever before. No flattening of careers is apparent here, despite all the talk about the death of apprenticeship. Senior members, more than at any time in the last 40 years, are carrying the legislative burden in Congress. This conclusion is not consistent with conventional wisdom, but conventional wisdom may have been hoodwinked by the same publicity machines that occasionally hoodwink constituents.

The measures of legislative involvement employed here are admittedly crude, but they come closer to tapping the concept than reading press clippings; and these measures clearly indicate that senior members make the legislative process go. What's more, the integrated model shows that a redistribution of formal positions to junior members would in no way compensate for the loss of senior members. Even when formal positions are held constant, increasing tenure is strongly and positively related to legislative activity as well as to legislative specialization and efficiency. Tenure in and of itself helps members to be involved legislative players, and this situation definitely did not go into abeyance in the 1980s. A subcommittee chair does not a legislator make.

The promise at the outset was that longitudinal analysis of this sort affords a unique and valuable perspective on modern congressional service. What does this perspective reveal? That what *can* be effectively routinized *has* been. Constituency service operations hum along from the start to the end of the modern congressional career. But legislative involvement, even though opportunities are now available for junior members, is primarily the province of their senior colleagues. Thus, many portions of the congressional career have lost their cyclical component and are now predominantly flat. If members move at all, it is only to maintain their relative position "in the pack." But the pure legislative component of congressional careers is more dependent on tenure than ever. Proficiency in this arena, it would seem, cannot be attained merely by attending workshops or by acquiring subcommittee positions but only through time and experience. On the basis of this evidence, mandatory term limitations would seem to be a bad idea. At the least, advocates of this proposal must be willing to accept the very real diminution in legislative capabilities that would result from the statutory exclusion of senior members. Better ways exist to stimulate congressional turnover than to force into exile those who are in many respects the most valuable legislators.

Notes

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1. A more detailed discussion of the procedures used to generate the required data, as well as of the implications surrounding mortality effects and the use of standardized scores is contained in my book *Congressional Careers* (1991), from which this article has been drawn.

2. I set aside the literature pertaining to the "sophomore surge." These works have a different purpose and are only concerned with a single election pair.

3. Besides mean share of the vote, a second widely used measure of electoral support is simply the percentage of incumbents who were reelected. The analysis was repeated using percentage successful and the same flattening of electoral careers is again evident. In fact, the line of best fit for percentage reelected in the most recent decade is actually slightly negative. This means that the chances of being defeated do not decrease with tenure in the modern House.

4. See n. 3.

5. This conclusion also holds when the analysis is confined to nonsouthern portions of the country. Thus, it is not the product of complications introduced by the breakup of the one-party South.

6. Further details on the specifics of the survey and its results can be found in Stubben and Hibbing 1987.

7. Representatives from the seven states closest to the District of Columbia have been removed from the analysis.

8. What about actual, rather than relative, travel? Over the last 8 to 10 years the frequency with which members have gone home has dropped dramatically (by over 25%). This is undoubtedly because office accounts were consolidated, so that reimbursement for trips home now reduces what members can spend on other aspects of the congressional office. Early research (see esp. Parker 1986a; 1986b) missed this point because its data base stopped in 1980. This decrease reversed a sharp increase that was due to alterations in the maximum number of reimbursable trips. The curvilinear pattern is actual travel home in the last 30 years makes it difficult to view life cycle patterns with absolute data, but it appears that these general conclusions still apply with the unstandardized data.

9. The variables in the model have been described previously with the exception of constituency attention and legislative involvement. Constituency attention is a combination of travel home and the percentage of staffers assigned to district, rather than Washington, offices (again, both standardized). However, travel home has now been adjusted for the distance of the representative's state from Washington. This adjustment was unnecessary before when the issue was *change* in travel home; but now the data run across careers and also across members, so distance must be taken into account. Legislative involvement is a linear combination of the index of legislative activity, the measure of specialization, and the measure of legislative efficiency.

10. All these statements apply to actual (unstandardized) as well as relative (standardized) data with the exception of sending staffers home. Because of major period effects (mean percentage of staffers assigned to the district went from 12% in 1959 to 43% in 1985), this particular measure indicates that over careers members increased their absolute attention to the district even as their relative attention diminished.

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THE POLITICAL ECONOMY OF PORK: PROJECT SELECTION AT THE U.S. ARMY CORPS OF ENGINEERS

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In previous studies of distributive politics scholars have investigated legislative influence without accounting for the policies' independent merits. As a result, they have failed to include a plausible explanation of the counterfactual (i.e., which projects would have been funded in the absence of congressional committee influence), which has led to invalid inferences regarding legislative influence. The model of distributive politics is reformulated to account for an assumed efficient and/or equitable project allocation in the absence of legislative influence. Using data from proposed Army Corps of Engineers' projects and the funding recommendations of three institutions, the findings indicate that pork barrel politics indeed exists and imposes significant efficiency costs but that both equity and economic efficiency play prominent roles in the decision-making process as well. Cost-benefit analysis is seen to play a constructive role by improving the efficiency of project choice; and the corps's cost-benefit analysis guidelines are beneficial from the agency's organizational perspective, as well.

A number of researchers have purportedly shown that self-interested legislators can affect policy outcomes in their favor. Legislators are assumed to be rational utility maximizers seeking only reelection. In order to be reelected, legislators are assumed to "look out for their own" by supporting projects that bring net benefits to their districts, even if the projects are inefficient from a national perspective. Local beneficiaries have an incentive to reelect such legislators, since the locality bears only a small fraction of project's costs while reaping a lion's share of the benefits.[1]

Tip O'Neill's reference to the legendary House Speaker characterizes the essential nature of distributive (or pork barrel) politics: "Sam Rayburn could make a call and the Army Corps of Engineers would go to work. Rayburn would take care of the little detail of an appropriation later" (Matthews 1988, 50). Evidence suggests that legislators channel federal projects to their constituents in forms ranging from high-tech science (Wilson 1989) to water resource projects (Drew 1970). Public works projects have been singled out as especially pork-laden, perhaps most notable among them the Army Corps of Engineers (see, e.g., Maass 1951; Ferejohn 1974).

The studies detailing alleged congressional excesses at the expense of the public interest have, however, suffered a critical flaw: they have muddled the important distinction between the political decision to fund a project and the project's estimated ex ante social value. Previous analyses have failed to specify a plausible counterfactual to distributive politics: What policies would have been enacted in the absence of congressional influence? In addition to improving a legislator's

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chance of reelection, a project may also provide net social benefits. Nevertheless, the relative merit of one project compared with another has been ignored in previous research. At the very least, some measure of a project's social value should be incorporated in any empirical analysis of *pork.* A further deficiency of previous studies, related to the first, is that although several find evidence of congressional pork, none attempt to measure the economic significance of the loss. As a result, policy makers and researchers alike provide little justification for widespread public concern over pork barrel politics.

I try to improve on past analyses in several ways. First, the decisions of three different institutions (the Army Corps of Engineers, the administration, and Congress) are analyzed to determine the degree of conformity between, for example, the administration and Congress in making funding allocations for new-start construction projects at the U.S. Army Corps of Engineers. Second, measures of project efficiency and equity are included in order to specify a plausible counterfactual to the theory of distributive politics and to determine the effects of key committees on project decisions independent of widely accepted policy objectives. Third, the efficiency of projects funded by Congress is compared with alternative objectives to determine the social cost of legislative influence over the Army Corps of Engineers. Finally, simulations of the decision process provide insight into the relative impact on project efficiency of considerations such as cost-benefit analysis and political influence.

I shall evaluate prior attempts to test for pork and offer a definition of pork that explicitly takes equity and efficiency into account. I shall introduce a model of legislative behavior that incorporates both legislative self-interest (i.e., reelection) and social welfare motives (e.g., efficiency and equity) for three different decision makers: the Corps of Engineers, the administration, and Congress. Data gathered on proposed new corps projects will be used to examine patterns implicit in actual policy decisions and to determine whether the process is characterized by pork. I shall compare the efficiency of the projects selected for funding with project rankings based on other criteria and provide an estimate of the welfare costs of the funded corps's projects as well. In addition, I shall examine the rates with which efficiency, equity, and political advantage are implicitly traded off with one another in funding new-start construction projects. Finally, I shall evaluate the role of cost-benefit analysis in improving the efficiency of corps projects.

What is Pork?

Essentially, there are two types of pork. The first, *programmatic pork,* is when Congress authorizes broad programs (e.g., sewer grants, water resource projects) so that legislators can show voters the tangible fruits of their labor (e.g., through ribbon-cutting ceremonies). In this case, the whole program is inefficient from a national perspective but has certain distributive properties attractive to members of Congress. The second type, *allocative pork,* takes as given the existence of a federal program, efficient or not. Allocative pork is when certain legislators, by dint of their committee positions, gain funding for their districts at the expense of more deserving districts. Allocative pork imposes social costs not because the entire program is necessarily inefficient (though it may be) but because the allocation of funds decreases social welfare.[2] I shall address only the latter type in this paper.

Considerable confusion exists over the definition of pork. Shepsle and Weingast contend that "the pork barrel consists of *inefficient* projects—those for which benefits do not exceed costs" (1981, 107;

Table 1. A Project's Potential Economic Impact

Case	National Benefits	Regional Benefits	Extraregional Benefits	Comments
1	+	+	+	All actors want (e.g., early warning defense systems)
2	−	−	−	No actors want (not likely to exist)
3	+	−	+	External benefits > concentrated costs (e.g., prisons)
4	−	−	+	Concentrated costs > external benefits (e.g., nerve gas storage facility)
5	−	+	−	External costs > concentrated benefits (e.g., inefficient sewage treatment facility)
6	+	+	−	Concentrated benefits > external costs (e.g., efficient water resource project)

Note: + = positive net benefits, − = negative net benefits. Although there are 8 (2 × 2 × 2) combinations, the two other cases of projects not mentioned are technically impossible: (1) where national benefits are positive but negative for both other categories and (2) vice versa.

original emphasis). Andrews's definition is "the practice of seeking expenditures of federal funds to benefit one's own region or congressional district at the expense of the country as a whole" (1979, 51n). In a *Washington Post* article, Paul Blustein described pork as "that which a member seeks for his own state or district 'but would not seek for anyone else's constituency."[3]

This mix of definitions can be distinguished by examining a project's possible economic impacts. Assume, for simplicity, that three perspectives on "efficiency" exist: regional (R), extraregional (E), and national (N). (International externalities are assumed to be absent, as are interdependent utilities across regions.) The ex ante net benefits to each for any potential project can be represented as RB, EB, and NB, respectively, where RB + EB = NB.[4] Ignoring the value (but not the magnitude) of net beneifts, for any given project six economic outcomes are possible, as represented in Table 1. In cases 1 and 2, all three geographic perspectives are in favor of, and opposed to, the project, respectively. Though a project where all are made worse off is unlikely to exist, a project where all gain may include recognized public goods, such as an early warning defense system. Cases 3 and 4 represent different magnitudes of benefits and costs between jurisdictions, although only a case 3 project is likely to be politically feasible. Following Olson (1965) case 4 projects, with large concentrated costs, will likely bond the groups or regions most harmed into a formidable political opposition. A case 5 project is traditionally defined as pork, where only the region gains at the expense of the nation. An example may include an inefficient sewage treatment facility constructed for the benefit of one district with costs spread nationally. A project for which national and regional net benefits are positive (case 6) is less frequently discussed in the literature, although it may be typical of many "pork barrel" initiatives. In this case, the benefits are highly concentrated in the district, and the costs are spread nationally, although the country is, in a utilitarian or additive sense, better off than before the change. Thus, this type of project would not, according to Shepsle and Weingast's definition (case 5), be considered pork if it were adopted;

nor would it qualify under Andrews's definition (also case 5).

A case 5 project would, however, be included in Blustein's definition.[5] The problem with defining pork as "that which a member seeks for his own district but would not for others" is revealed when we pose the question, If every district were slated to receive such a project, would the country as a whole disapprove? In other words, is the sum of the individual projects efficient or not (assuming they are separable and additive in their benefits and costs)? If all projects have greater benefits than costs (with suitable compensation), all would be better off, and the program would win national approval. If all the projects are inefficient, then everyone loses and the program as a whole is unacceptable. Blustein's (case 6) definition relegates both efficient and inefficient projects to the pork barrel. This sets up the unusual situation where a rational majority could support one of Blustein's so-called pork barrel projects in every district! Invariably, some nationally funded projects will confer concentrated benefits to some regions; not all of them, however, represent pork.

On the other hand, the Shepsle and Weingast definition of pork (case 5) appears overly restrictive, since it could lead to the conclusion that selecting any project from a group of "nationally efficient" choices is acceptable. Using their definition, choosing a committee member's favored project with $25 million in net benefits over one conferring $100 million in net benefits might not be considered pork: both are efficient relative to the status quo. Since governments generally cannot fund all projects where benefits exceed costs, efficiency requires a rank ordering of projects by their benefit/cost ratios. Shepsle and Weingast's definition makes no such ordering necessary despite the efficiency implications. Moreover, they do not take into account other legitimate social criteria besides efficiency (e.g., redistributional equity) to justify policy choices.

I define pork as *geographic legislative influence extending beyond commonly accepted notions of social welfare (e.g., efficiency and equity)*. For these purposes, efficiency is defined broadly to include projects for which social benefits exceed all available alternative investment opportunities. (Unfortunately, this definition of efficiency is difficult to account for empirically, and I have had to narrow it somewhat in the empirical section to follow). Thus, efficient project selection would account not only for private sector alternatives but also for alternative government investment opportunities. This definition of pork is applicable for distinguishing project selection where most of the alternatives are those with measured national benefits that exceed costs. Pork barrel expenditures could include projects both efficient and inefficient relative to the status quo. The relevant determination is whether political influence remains after accounting for efficiency and equity considerations. On the other hand, selecting an "inefficient" project may not represent pork if there are equity or other public justifications for its existence. Other grounds for project selection must be accounted for before unambiguously identifying pork.

Legislative Influence over Agency Decisions

A number of studies have documented positive correlations between committee membership and agency policies, concluding that a causal relationship is present (e.g., Arnold 1979; Crandall 1983; Ferejohn 1974; Goss 1972; Weingast and Moran 1983). There is, however, ample room for skepticism. A primary problem is that a simple correlation between project funding and committee membership fails to identify the direction of causality: Does committee membership influence the project selection process (the classic pork barrel), or does the district's need for the project drive committee assignments, or both? This is the heart of the issue of whether Congress chooses projects based on legislative self-interest or on its perception of the national interest.[6]

Frequently overlooked is the possibility that the committee–project correlation could be the result of a legitimate process. Legislators could be serving on the relevant committees simply because their constituents, using acceptable social welfare criteria, should be granted those projects. Legislators thus select themselves for such appointments, in a manner that may be nationally beneficial as well as furthering their reelection efforts. An observed correlation between committee membership and project funding could be the result not of the inherent political influence of these positions but rather an indication of the regions most deserving of certain federal projects.

On the whole, legislators' preferences probably reflect both political and public interest objectives. Legislators may still be preoccupied by reelection goals; but, having sufficiently satisfied their constituencies, they may pursue national objectives, which may include efficiency or equity in federal programs. There is descriptive evidence that legislators actively pursue goals other than reelection (see, e.g., Ashball 1978; Drew 1979).[7] There is not, however, any evidence establishing whether or how legislators trade off public interest and political objectives.

In the absence of an independent measure of efficiency, the existing utility-maximizing theory provides little guidance for distinguishing between nationally inefficient pork and "efficient" pork (cases 5 and 6). Indeed, the "rational" legislator would be indifferent to the project's national efficiency consequences, trying, instead, only to direct as many jobs and as much income as possible to his or her own constituents.[8] By failing to include measures of project merit, past studies in effect deny the possibility of public interest motives on the part of legislators.

A Reformulated Model of Distributive Politics

The basic issue confronting the study of distributive politics is explaining the geographic distribution of discrete federally funded projects with concentrated benefits financed by general revenues. The fundamental problem with all such models is that the counterfactual (i.e., which projects would have been funded in the absence of self-interested legislative influence) is unknown. Indeed, many empirical analyses have failed to recognize any alternative to distributive politics.

The absence of knowledge concerning the counterfactual does not prohibit model testing designed to simulate the allocation of federal projects, however. One plausible assumption is that in the absence of congressional committee influence, projects would be distributed based on their relative efficiency. With a federal budget constraint, projects with the highest benefit/cost ratios would be funded sequentially until the money allocated to the program was exhausted. This admits the possibilities that (1) not all projects with benefit/cost ratios exceeding unity will be funded, and (2) some projects with benefit/cost ratios below one may be funded (i.e., all the money for the federal program must be expended).

Allowing the counterfactual that projects will be distributed efficiently (given budgetary and program constraints), we would then expect relevant congressional committees to exert influence over the geographic distribution of projects. Legislators are assumed to seek reelection by providing local benefits for their constituents. Specifically, if two legislators from otherwise equivalent districts are vying for a set of otherwise identical projects, we would expect the one with the relevant appropriations or oversight committee assignment to reap a greater share of the projects than the legislator with no relevant committee responsibility.

With few exceptions, corps projects are authorized by Congress for new construction before money is appropriated. The budget process for Army Corps of Engineers new-start project proposals essentially entails three steps: (1) the corps recommends a set of new projects for the president's budget proposal, (2) the president's annual budget proposal includes a set of projects, which may not coincide with corps recommendations, and (3) Congress approves a public works spending package that frequently includes appropriations for new corps projects.

The relevant consideration is whether political influence remains after accounting for the social merits of the proposed project. Because the distributive politics model cannot be tested adequately without a public interest component, the model of distributive politics used in this study therefore integrates elements of the public interest conception. The distributive politics model holds that the likelihood of funding corps projects is based on political congressional committee influence (P) and the impact on the federal budget (B). The reformulated model adds the possibility that Congress and executive agencies may display public interest motives, including efficiency (E) and equity (Q). Thus, the integrated model of public interest and distributive politics is $\text{prob}_i = f(E_i, P_i, Q_i, B_i)$, where prob_i is the probability of choosing proposed project i based on the vector of project attributes, which include its economic effect (E_i), political payoff (P_i), redistributive equity (Q_i), budgetary impact (B_i), and other attributes expected to affect project selection.[9]

The three decision processes modeled for the corps's projects represent the three budgetary stages: (1) corps budget recommendation $= f(E, P, Q, B)$, (2) president's budget $= g(E, P, Q, B)$, and (3) congressional funding $= h(E, P, Q, B)$. The model allows us to determine not only which objectives are important to each decision process but also the rates with which economic efficiency is implicitly traded for political expediency, equity, and budgetary concerns. In addition, it allows testing for systematic differences in the decision processes across the three institutions.

The Data

The data include a sample of 133 potential corps projects previously authorized and eligible for appropriations. The data are limited to new-start construction projects authorized by Congress in the Water Resources Development Act (WRDA) of 1986 (Public Law 99-662),[10] whose cost-benefit studies had been completed between 1974 (to conform with the updated 1973 *Principles and Standards*[11] and 1983 (to ensure that the project would be ready for a construction appropriation).[12] Most projects addressed problems related to flood control and river and harbor navigation. A number of projects authorized in the 1986 act were not included in the sample either because cost-benefit calculations were not performed (such as for environmental or wildlife mitigation projects) or because feasibility studies were completed before 1974 or after 1983. While the 133 potential projects studied represented only one-half of those authorized in 1986, they accounted for 65% of total costs and 82% of federal costs of all authorized projects.[13]

Three binary dependent variables are included in the logit equations, each representing a different policy decision. The first variable is whether or not the corps recommended the project to the Of-

Table 2. Number of Corps Projects by Outcome Type

	Type								Number of Projects	Percentage of Total
Decision	1	2	3	4	5	6	7	8		
Corps funding recommendation?	—	—	—	—	X	X	X	X	19	14.3
President's budget?	—	—	X	X	—	—	X	X	27	20.3
Congressional appropriation?	—	X	—	X	—	X	—	X	31	23.3
Number of projects	100	5	0	9	0	1	2	16	—	—
Percentage of total	75.2	3.8	0	6.8	0	.8	1.5	12.0	—	—

Note: X indicates a positive decision. N = 133.

fice of Management and Budget (OMB) for inclusion in the president's budget (*yes* = 1, *no* = 0). The second dependent variable is coded 1 if the project was included in the president's annual budget proposal in either 1985, 1986, or 1987 and 0 otherwise. The final variable is for congressional appropriations, coded as 1 for a favorable funding decision between 1985 and 1987 and 0 otherwise. From the sample of 133 new-start construction projects, 19 were recommended to OMB for construction by the corps in an internal administration document, 27 were included in the president's budget, and 31 received congressional appropriations. Table 2 shows the distribution of projects across the three decision loci. For example, a project of decision type 4 is one in which the corps did not recommend funding but the project was included in the president's budget and Congress appropriated money; 9 projects occupied this category. Most of the 133 projects were not funded, and 16 of the 31 that were funded were approved by all three decision makers.[14]

The 16 independent variables include data on the projects' economic, budgetary, political, and equity characteristics and are described more fully in Tables A-1 and A-2 of the Appendix. Table 3 reveals differences between the funded and unfunded projects. The most striking difference between the funded and unfunded projects is their relative size. The average total cost of the funded projects (local plus federal costs) is nearly twice as large as the average cost of the unfunded projects. And because the average benefit/cost ratio is larger for the 31 funded projects, their benefits are over four times as large as those for the unfunded ones. The federal cost, however, is only slightly over 40% larger for funded projects. The economic variables include each project's total costs (local costs [LOCCOST] + federal costs [FEDCOST]) and benefits (BENEFIT) estimated by the corps in their cost-benefit analyses (in discounted 1986 dollars).[15] The estimated federal cost of the project (FEDCOST) is included separately to represent the federal budgetary impact over the life of the project, also in discounted 1986 dollars. The political variables indicate the number of key legislators from the district or state where the project is targeted and the number of districts and states spanned by any given project. (The identities of key congressional committees were gathered from interviews with corps officials.) Finally, the project's potential equity impact, which assumes a simple motive of wealth redistribution, is represented by three variables: the state's per capita income (INC), the percentage of persons living below the poverty level in the state (POV) and, in order to capture relative local wealth, the ratio of the district's median housing value to the state's (RELHVAL).

The definition of efficiency used in this empirical analysis is somewhat narrower

Table 3. Mean Deviations for Funded and Unfunded Projects

Variable	Funded Mean (A)	Unfunded Mean (B)	Mean Deviation (A − B)	Units
Total benefits (BENEFIT)	599,445	140,519	458,926	$1,000s
Local costs (LOCCOST)	42,580	15,878	26,702	$1,000s
Federal costs (FEDCOST)	75,572	53,362	22,210	$1,000s
House Appropriations Subcommittee (HSAPP)	.45	.21	.25	legislators
House leader (HDLDR)	.03	.01	.02	legislators
Senate leader (SLDR)	.19	.07	.12	legislators
Senate Appropriations Subcommittee (SAPP)	.23	.31	−.09	legislators
House Armed Services Committee (HAS)	.23	.17	.06	legislators
Senate Armed Services Committee (SAS)	.61	.40	.21	legislators
House Appropriations Committee (HFAPP)	.35	.26	.09	legislators
Senate Appropriations Committee (SFAPP)	.52	.73	−.21	legislators
Number of districts spanned per project (NDISTS)	1.74	1.58	.16	—
Number of states spanned per project (NSTATS)	1.16	1.14	.02	—
Relative housing value (RELHVAL)	.96	.99	−.04	proportion
Average state per capita income (INC)	13,424	13,604	−180	dollars
Persons below poverty level (%) (POV)	13.91	12.66	1.25	percent
Number of cases	31	102		

than that defined above for determining pork. For the empirical analysis, one project is said to be more efficient than another if its net benefits are greater or, given the overall budget constraint, if the benefit/cost ratio is higher. Thus, given the existence of the political process that determined the set of projects to choose from, the most efficient policy is to begin selecting those corps projects with the highest benefit/cost ratios until the funds are exhausted. Although this is not necessarily the most efficient set of projects from which to choose, it reflects a more realistic picture of the set of choices confronting individual agencies than the more global definition of efficiency used earlier to characterize pork.[16]

Results

The model's three equations were estimated jointly by stacking the three binary decision variables as the dependent variable (N = 399, or three times the number of projects), with three sets of decision-specific dummy variables controlling each of the 16 independent variables (for a total of 48 variables). The estimation was performed in two ways. In the most general form, the coefficients for each decision process (congressional, administration, and the corps budget recommendation) are allowed to vary independently. This is equivalent to estimating each of the three equations separately. In the most restricted case, all of the coefficients are constrained to be equal across the three decision processes, and only 16 independent variables are estimated. With this jointly estimated model, systematic differences between the decision processes of the corps, the administration, and Congress can be tested. The results of the logit estimates are presented in Tables 4 and 5.[17] Immediately apparent from these results is the robust nature of the parameter estimates across the two model specifications[18] and the stability of the size and significance of the political variables.

Economic and Budgetary Effects

The project's total benefits (BENEFIT) are always significant and positive, implying

Table 4. Relationships between Project Characteristics and Decisions to Approve with All Coefficients Constrained

Variable	Logit Coefficient	*t*-ratio
Intercept	-11.81	-3.62
Total Benefits (BENEFIT) (billions of dollars)	1.57	4.46
Local costs (LOCCOST)	16.41	3.32
Federal costs (FEDCOST) (billions of dollars)	-7.89	-2.51
House Appropriations Subcommittee (HSAPP)	.72	1.83
House leader (HDLDR)	4.98	3.28
Senate leader (SLDR)	1.37	2.32
Senate Appropriations Subcommittee (SAPP)	-1.90	-2.83
House Armed Services Committee (HAS)	-.21	-.55
Senate Armed Services Committee (SAS)	1.53	3.34
House Appropriations Committee (HFAPP)	.30	.70
Senate Appropriations Committee (SFAPP)	-.47	-.83
Number of districts (NDISTS)	-.90	-3.37
Number of states (NSTATS)	2.58	3.58
Relative housing value (RELHVAL)	-1.69	-2.00
Average state per capita income (INC) (thousands of dollars)	.36	2.48
Persons below poverty level (%) (POV)	.34	3.49

N = 399; d.f. = 382; x^2 = 283.7; Log likelihood = −134.7; % correct = 87.2; LR index = .513

Table 5. Relationships between Project Characteristics and Decisions to Approve with All Coefficients Unconstrained

Type	Logit Coefficient	*t*-ratio
Intercept	-12.55	-3.66
Total benefits (BENEFIT) (billions of dollars)		
Congress	2.35	2.62
Budget	1.51	2.46
Corps	1.26	2.07
Local costs (LOCCOST) (billions of dollars)		
Congress	16.06	1.61
Budget	14.32	1.88
Corps	25.08	2.40
Federal costs (FEDCOST) (billions of dollars)		
Congress	-12.04	-1.87
Budget	-8.44	-1.61
Corps	-4.66	-.95
House Appropriations Subcommittee (HSAPP)		
Congress	.98	1.52
Budget	.75	1.08
Corps	.64	.74
House leader (HDLDR)		
Congress	6.07	2.08
Budget	4.84	1.88
Corps	4.88	1.89

TABLE 5 (continued)

Type	Logit Coefficient	*t*-ratio
Senate Leader (SLDR)		
Congress	2.05	1.96
Budget	1.99	1.89
Corps	.18	.14
Senate Appropriations Subcommittee (SAPP)		
Congress	-2.71	-2.27
Budget	-2.08	-1.77
Corps	-.75	-.58
House Armed Services Committee (HAS)		
Congress	.15	.24
Budget	-.00	-.00
Corps	-1.36	-1.50
Senate Armed Services Committee (SAS)		
Congress	1.26	1.61
Budget	1.95	2.35
Corps	1.73	1.87
House Appropriations Committee (HFAPP)		
Congress	.16	.21
Budget	.15	.20
Corps	.47	.53
Senate Appropriations Committee (SFAPP)		
Congress	-.35	-.40
Budget	-.24	-.26
Corps	-1.17	-1.07
Number of districts (NDISTS)		
Congress	-1.02	-2.01
Budget	-.89	-2.00
Corps	-.99	-1.85
Number of states (NSTATS)		
Congress	2.89	2.48
Budget	2.64	2.40
Corps	2.68	2.02
Relative housing value (RELHVAL)		
Congress	-2.44	-1.56
Budget	-1.14	-.84
Corps	-1.82	-1.11
Average state per capita income (INC) (thousands of dollars)		
Congress	.36	2.15
Budget	.38	2.23
Corps	.47	2.53
Persons below poverty level (%) (POV)		
Congress	.47	3.76
Budget	.31	2.51
Corps	.25	1.88

N = 399; d.f. = 350; $x^2 = 299.9$; Log likelihood = −126.6; % correct = 87.0; LR index = .542

that projects with large benefits are more likely to be favored at all three decision levels. Total project costs (LOCCOST + FEDCOST) are negatively (but insignificantly: *t*-ratio = −1.02) related to funding probability, suggesting that a project's economic costs cannot here be viewed as political benefits independent of measured economic benefits. The benefit/cost ratio (BENEFIT/total costs) is negatively related to project funding under all specifications, though it is statistically significant only once (for the corps) and insignificant for the restricted model as well. This suggests that efficiency does not dominate the decision-making process.[19] Therefore, the results of these equations with respect to efficiency are unclear. While benefits contribute positively and total costs (weakly) negatively to project funding, the benefit/cost ratio is unexpectedly negatively correlated. Taken as a whole, however, the three economic variables have a statistically significant impact on the model's parameters.[20]

The project's federal cost (FEDCOST) consistently enters the estimated functions negatively, and significantly for the restricted model, implying, as expected, that higher federal costs detract from a project's acceptability. However, despite a history of arguing over the cost-sharing arrangements between the local and federal governments (on average the federal share of project costs was 67% for the 133 projects involved), the federal share of total project costs was not significant in any model specifications run (not shown). This indicates that despite some reluctance to spend federal money, there is little hesitancy on the part of decision makers to foot a large share of the bill once projects are approved.

Political Effects

The Senate Armed Services Committee (SAS) appears to play a central role in gaining projects for their members due to the corps's military affiliation.[21] In addition, the model's coefficients for the House Appropriations Subcommittee (HSAPP), House leadership (HDLDR), Senate leadership (SLDR), Senate Armed Services (SAS), House Armed Services (HAS), and the full Appropriations committees, though not always statistically significant, were, with only two exceptions, positive. While generally large, stable, and, as expected, positive, the political variables include one anomaly: Senate Appropriations Subcommittee (SAPP) membership contributed negatively, and sometimes significantly, to project funding, particularly in the congressional funding decision.[22] Taken as a whole, the most influential political positions in delivering corps projects were the House and Senate leadership and Senate Armed Services Committee assignments, both of which were strongly significant and positive. Somewhat surprisingly, the number of districts the project encompasses (NDISTS) detracts from the likelihood of project funding for all three decision-making institutions. On the other hand (and as expected), the coefficient on the number of states (NSTATS) spanned by the project is large and positive: more states involved suggests a significantly higher probability of project funding.[23] Evaluated together, the 10 political variables have a statistically significant impact on project funding decisions independent of the projects' efficiency and equity impacts.[24]

Equity Effects

The three equity variables had a statistically significant impact on the funding decision patterns, just as we observed for the economic and political variables.[25] The poverty level of the state in which the project is targeted (POV) enters each equation positively (the higher the poverty level, the more likely is project funding), implying a possible redistributive concern on the part of all three decision-making institutions. However, the model's implications with respect to equity are mixed: while, as expected, the poverty level (POV) is positively related to project funding and while the relative housing value (RELHVAL) is negatively related (the higher the relative value of a district's housing to the state's, the less likely the project will be funded), income (INC) is positively and significantly related to funding outcomes across all specifications. Although income is most significant for the corps's budget recommendation (and is larger as well), the positive coefficients indicate in all cases a mixed concern for equity by all three decision-making bodies. One explanation for a positive income coefficient is that wealthier regions will be more able to contribute the necessary funds for project completion than poorer ones, thus increasing the probability that if federal money is committed, the project will be completed and maintained effectively by local sponsors. It is also possible that the redistributional motives ascribed here to the positive "equity" coefficients may be explained by the hypothesis that legislators from poorer districts will lobby harder for federal funds than representatives from wealthier ones, since the former may have little else of importance to offer their constituents.

Decision-making Patterns

Inferring systematic patterns from public decisions at various levels of governments is, to say the least, complex. Such decisions reflect the expectations of how the other agency or branch will respond as well as the preferences of the decision makers being examined. For example, the list of corps projects included in the president's budget proposal can be seen either as an initial offer in the bargaining that will ensue with Congress or as the result of frequent behind-the-scenes negotiations with Congress. But given the "dance of legislation" that accompanies decisions on corps projects, the revealed preferences of the congressional and administration decisions are remarkably similar. The values of their likelihood functions are statistically indistinguishable, and the signs are in the same direction.[26] The administration appears to display an awareness of congressional committee membership and leadership positions as revealed in its budget proposal. Although the corps may not become involved directly in recommending projects based on political factors, either congressional influence is strongly felt in the White House on a project-by-project basis or vice versa, or else the influence is reciprocal.[27]

Somewhat surprisingly, taken as a whole there are no systematic differences in the model between the three decision-making institutions.[28] Estimating other equations (not shown) that allow coefficients of selected variables to vary, one finds no systematic differences from one decision maker to another. These include specifications allowing independent variation in the coefficients for project benefits, local costs, federal costs, the benefit/cost ratio,[29] all political variables, and equity coefficients (RELHVAL, INC, and POV). Although systematic differences between decision makers were largely insignificant, there were substantial differences in the weights accorded the various economic, budgetary, political, and equity factors.

Estimated Elasticities and Marginal Rates of Substitution

While the sign and significance of the coefficients is important for interpretation, it is also instructive to display the relative magnitudes of each coefficient. Estimates of the implied elasticities of the independent variables, evaluated at the means of each variable, are shown in

Table 6. Logit Elasticity Estimates

Variable	Elasticity Congress	Elasticity Budget	Elasticity Corps	Mean ($)
Total benefits (BENEFIT)	.51	.33	.29	$247,490
Local costs (LOCCOST)	.31	.28	.52	$22,102
Federal costs (FEDCOST)	-.61	-.44	-.26	$58,540
House Appropriations Subcommittee (HSAPP)	.22	.17	.16	.26
House leader (HDLDR)	.11	.09	.09	.02
Senate leader (SLDR)	.18	.18	.02	.10
Senate Appropriations Subcommittee (SAPP)	-.68	-.53	-.20	.29
House Armed Services Committee (HAS)	.02	.00	-.23	.18
Senate Armed Services Committee (SAS)	.49	.78	.73	.45
House Appropriations Committee (HFAPP)	.04	.04	.13	.29
Senate Appropriations Committee (SFAPP)	-.21	-.14	-.74	.68
Number of districts (NDISTS)	-1.43	-1.28	-1.50	1.62
Number of states (NSTATS)	2.91	2.70	2.91	1.14
Relative housing value (RELHVAL)	-2.06	-.99	-1.67	.98
Average state per capita income (INC)	4.35	4.73	6.12	$13,562
Persons below poverty level (%) (POV)	5.40	3.59	3.14	12.95

Note: Induced percentage change in probability of project funding based on 1% increases in the means of the independent variables.

Table 6.[30] One can see that while the economic variables are not unimportant, the equity components (relative housing value, income, and poverty levels) are particularly powerful in funding decisions, as are the number of states and districts spanned by the project. The ordinary least squares elasticity estimates (not shown) reveal patterns similar to the logit elasticities.

One of the chief advantages of the model's multiattribute specification is the ability to provide estimates of the relative worth of—or willingness to trade—one attribute with another. How much in benefits is a House leadership position worth to project funding? How are the relative housing values and poverty levels balanced against one another by congressional, administration, and corps decision makers? Answers to these questions can be expressed by calculating the marginal rates of substitution (MRS) implied by the logit coefficients.

The MRS displayed in Table 7 show the rates at which one variable is substituted for another at the observed probabilities. For example, the ratio of the FEDCOST coefficient to BENEFITS is −5.12, implying that a $1.00 reduction in federal costs has roughly the same (positive) impact on the probability of congressional funding as an additional $5.12 of project benefits. A similar interpretation can be made for the funding decision by the president ($5.59), the corps's budget recommendation ($3.70), and the three decisions modeled jointly ($5.03). These results suggest that benefits are, at the margin, far less significant than budgetary costs in funding decisions (as can be expected during lean budgetary periods).

Other comparisons from these tables are instructive as well, particularly with respect to the political variables. The single most influential political position in securing corps projects is a House leader from the same district as the project. This has roughly the same impact as an additional $3.2 billion in project benefits and has more than twice the impact of any other political position. Its relative strength is even more pronounced when compared with the equity variables. One

Table 7. Marginal Rates of Substitution for Project Recommendation, by Decision Type

Variable	Decision Type Combined[a]	 Congress	 Budget	 Corps
Total benefits (BENEFIT)	1	1	1	1
Local costs (LOCCOST)	10.45	6.83	9.48	19.90
Federal costs (FEDCOST)	(5.03)	(5.12)	(5.59)	(3.70)
House Appropriations Subcommittee (HSAPP)	458,599	417,021	496,689	507,937
House leader (HDLDR)	3,171,975	2,582,979	3,205,298	3,873,016
Senate leader (SLDR)	872,611	872,340	1,317,881	142,857
Senate Appropriations Subcommittee (SAPP)	(1,210,191)	(1,153,191)	(1,377,483)	(595,238)
House Armed Services Committee (HAS)	(133,758)	63,830	(1,999)	(1,079,365)
Senate Armed Services Committee (SAS)	974,522	536,170	1,291,391	1,373,016
House Appropriations Committee (HFAPP)	191,083	68,085	99,338	373,016
Senate Appropriations Committee (SFAPP)	(299,363)	(148,936)	(158,940)	(928,571)
Number of districts (NDISTS)	(573,248)	(434,042)	(589,404)	(785,714)
Number of states (NSTATS)	1,643,312	1,229,787	1,748,344	2,126,984
Relative housing value (RELHVAL)	(1,076,433)	(1,038,298)	(754,967)	(1,444,444)
Average state per capita income (INC)	299.30	153.19	251.66	373.02
Persons below poverty level (%) (POV)	216,561	200,000	205,298	198,413

Note: Ratio of selected logit coefficients to BENEFIT coefficient. Numbers in parentheses indicate negative values. The MRS between other pairs can be found by calculating the ratios between the pairs above.

[a] All coefficients are constrained across the three decisions.

House leader from the project's district has roughly the same estimated impact on project funding as (1) an increase of over $13 thousand in average state income; (2) a rise in the poverty rate of 14.6 percentage points; and (3) 1.9 additional states covered by the project, the first two of which, when evaluated from the mean, exceed the highest observed levels.

Equally prominent is the ratio of the effects of project benefits and costs.[31] A $1.00 increase in the benefits of a project has the same (positive) marginal impact on project funding as a reduction of $.77 in total costs, illustrating the relative significance of benefits over costs. Although on efficiency grounds a $1.00 reduction in costs is equivalent to a $1.00 gain in benefits, in practice increased project benefits appear to generate greater political viability than reductions in costs. In addition, viewing differences between equations, the corps appears to attach far greater significance to local costs and far less to federal costs than either Congress or the administration.

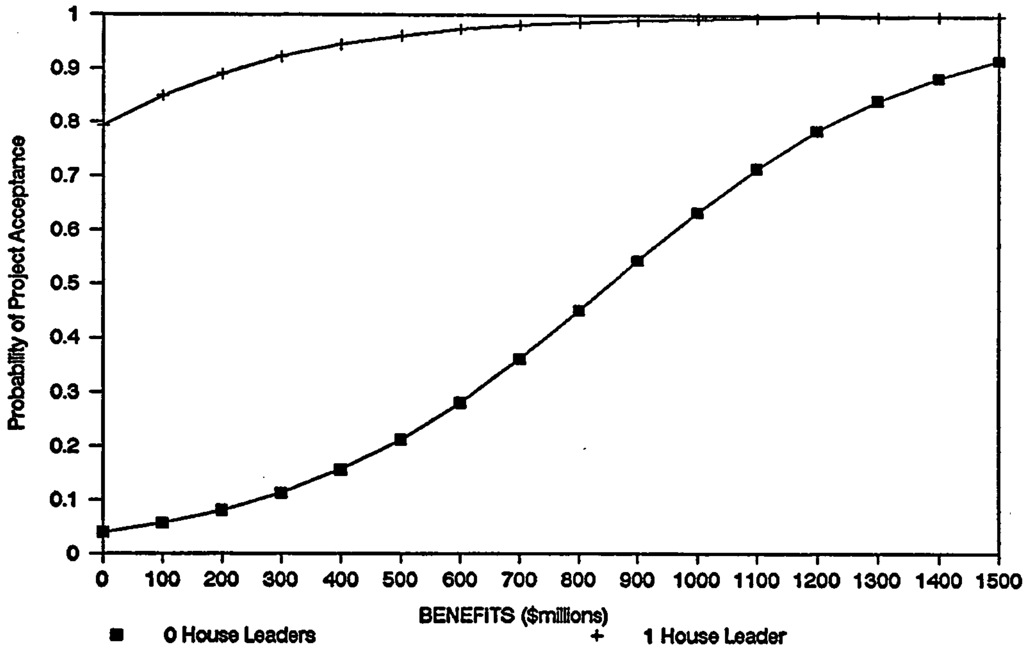
Predicted Probabilities

The relative importance of each variable can be displayed more intuitively by graphing the implied changes in funding probability based on changes in one of the independent variables. Several of these changes are displayed in Figures 1–4.[32]

Figure 1 shows the strong influence that a House leader from the project's district has on project approval. Evaluated at the means, the probability of funding is approximately 10% without a House leader, and 90% with one House leader. The level of benefits has a small relative impact; even as the benefits of the project approach zero, the probability of project funding remains nearly 80% with a House leader representing the project.

The other political variable shown is the impact of the number of states

Figure 1. House Leadership As Predictor of Project Acceptance

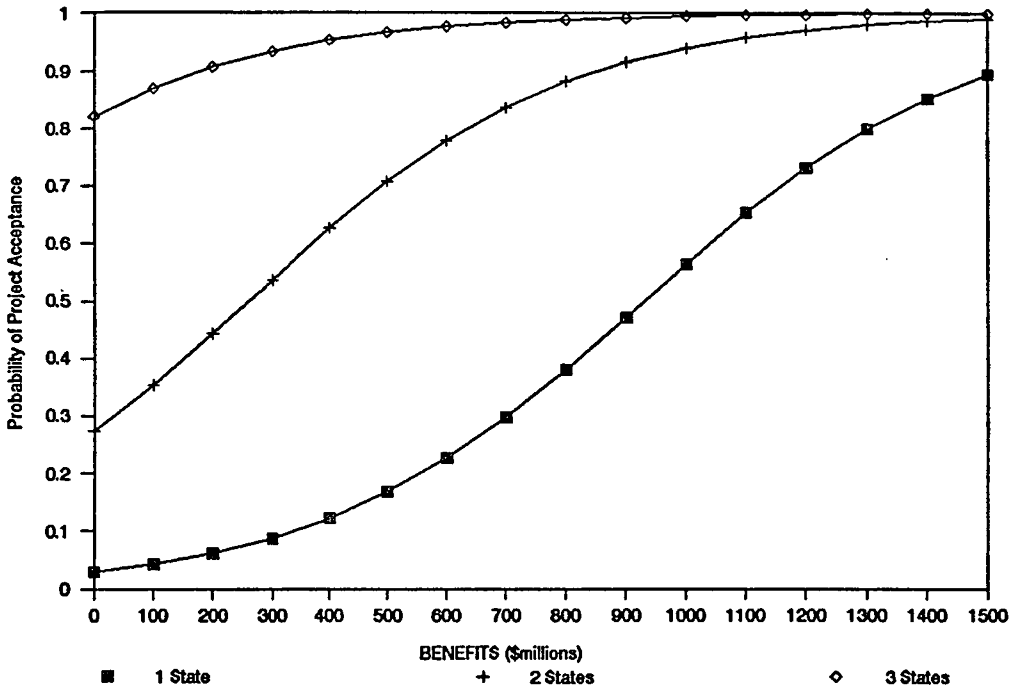


spanned by the project on project acceptance (Figure 2). Not only can we see the powerful influence of projects crossing state borders, but their growing marginal impact as well. While an "average" project in two states still maintains an acceptance probability below one-half, a third state raises the probability to over 90%. It appears that any large project in at least two states is virtually assured of project funding, even without extraordinary economic or political support.

Figure 3 shows probability changes induced by poverty levels between 10% and 20%, indicating that marginal changes are especially pronounced at higher poverty levels. While at a 10% poverty level the probability of funding is less than 5% (evaluated at the means), the probability is still below 20% when the poverty level rises to 15% of the state's population. However, an additional five percentage points of poverty raises the funding probability to nearly 50%. Except for very large projects (those with over $700 million in benefits), the marginal impact of rising poverty levels increases, suggesting a greater willingness to help particularly depressed areas rather than areas with moderate levels of poverty.[33] Again, the relatively "flat" curve, even with benefits graphed in hundreds of millions, shows the relatively small marginal influence of benefits on project funding.

The impact of federal costs on project funding, depicted in Figure 4, indicates a more stable marginal effect. However, even at the relatively low federal cost of $10 million, an "average" project has less than a 30% chance of being funded. The graph also shows the diminishing marginal political cost of federal funds. Particularly at lower (and more representative) benefit levels, the distance between the low federal cost curve ($10 million) and the middle curve ($60 million) indicates

Figure 2. Number of States As Predictor of Project Acceptance



that the first $50 million has a much larger (negative) effect on project funding chances than does the next $50 million. In other words, the marginal disutility of spending federal dollars appears to decrease with size, at least over the range observed.

Policy Implications

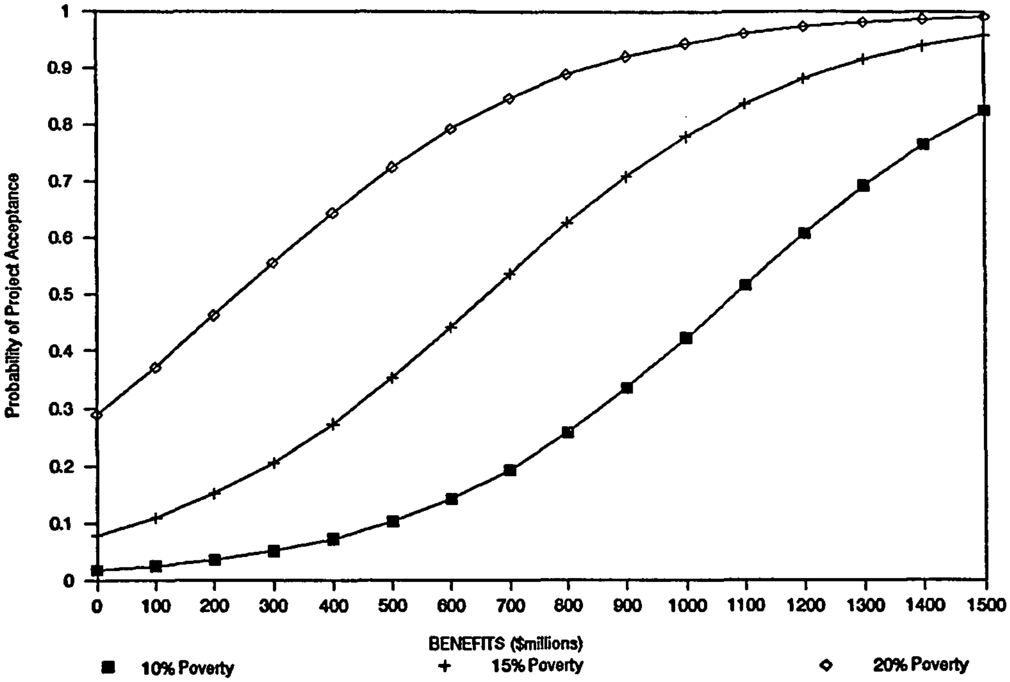
How Costly Is Pork?

Much of the literature on congressional influence over agency decisions simply measures (albeit unsatisfactorily) whether or not such leverage exists. While it is important to understand the nature of the influence, the policy consequences remain unknown. A critical factor is the magnitude of the inefficiency induced by distributive politics, not merely the finding that political influence exists. While knowledge of the marginal effect of project attributes on funding decisions is instructive, for policy purposes the greatest concern involves the efficiency of the entire set of projects selected. I attempt to measure the economic cost of political influence over corps projects and compare the efficiency effects of various hypothetical objective functions with that of projects actually funded.

The 31 projects selected by Congress for appropriations have a total lifetime cost of $3.66 billion, with over $18 billion in total benefits measured using the corps's cost-benefit analyses. By comparing other ways of spending the $3.66 billion in total project costs, restricted to this sample of 133 potential new starts, we can determine the efficiency of various objective functions compared with the actual project mix.[34]

Under certain restrictions,[35] the most

Figure 3. Percent Living Below Poverty Level As Predictor of Project Acceptance



efficient way to select projects is to rank them in terms of their benefit/cost ratios. Selecting from the 133 potential corps projects, an efficient project mix includes 33 projects (2 more than actually funded) with net benefits roughly $3.6 billion larger than the actual selection. This is the baseline "efficient" set of projects against which the other objectives are compared. The first row of Table 8 shows this "efficient" mix of projects, whose total costs are nearly the same as the actual project selections, while the second row displays the efficiency consequences for projects actually funded. Because the projects are discrete, the total project costs are not precisely equivalent.

If Congress had chosen to fund the projects not on efficiency grounds but rather based solely on political criteria (measured by the sum of the number of members from the House and Senate Appropriations subcommittees and leadership positions who represent a particular project), the efficiency loss would rise to over $5.3 billion. In other words, the "price" society would pay for such political benefits would be $5.3 billion in net benefits. If, instead, Congress were to adopt only an equity objective in an attempt to redistribute money from wealthier areas to poorer ones, the efficiency losses would be even higher. Using three different measures of equity, Table 8 shows the efficiency costs of each, which range from $7.7 to $12.2 billion.

If federal budgetary considerations are of primary importance, ranking projects based on the federal share of the project's costs (from low to high) will produce the lowest possible federal cost. Reducing the federal cost from the actual $2.34 billion

Figure 4. Federal Cost As Predictor of Project Acceptance

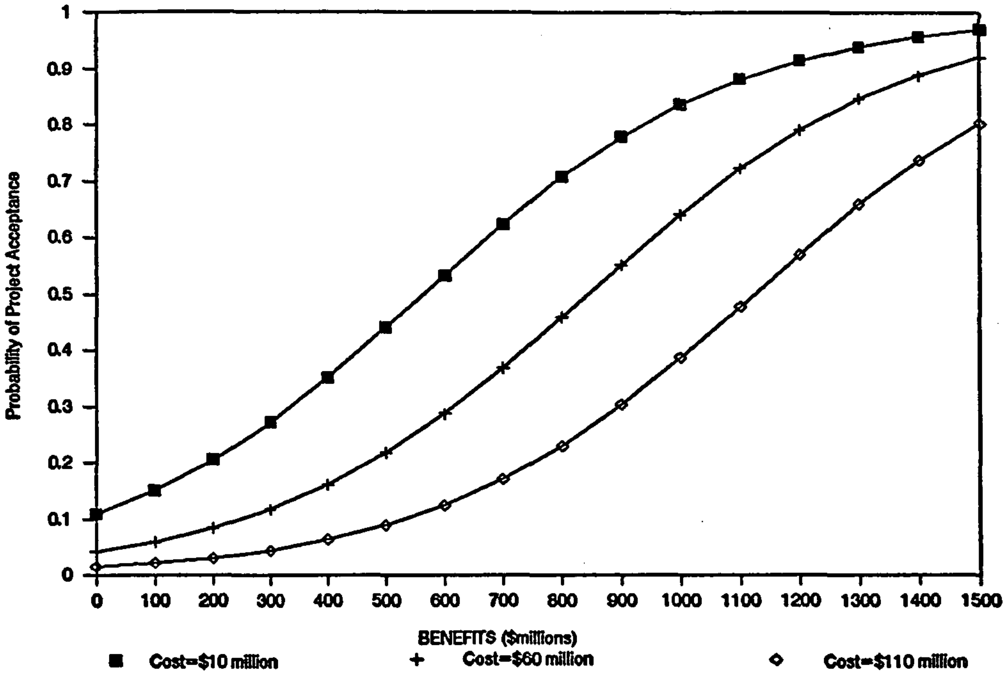


Table 8. Efficiency of Various Objectives

Objective	Number	Total Benefits	Federal Cost	Net Benefits	SOCIAL COST[a]
Efficiency	33	22,159	2,265	18,500	0
Actual	31	18,583	2,343	14,920	3,580
Political[b]	25	16,764	2,714	13,141	5,359
Equity					
Persons below poverty level (POV)	31	13,998	2,721	10,460	8,040
Relative housing value (RELHVAL)	53	9,881	2,558	6,331	12,169
Average state per capita income (INC)	34	14,489	2,792	10,844	7,656
Budgetary	47	12,713	1,805	9,073	9,427
Average project	45	11,137	2,634	7,508	10,992
Model					
All variables	28	19,184	2,285	15,541	2,959
Without economic	104	7,451	2,401	3,800	14,700
Without political	11	18,879	2,291	15,227	3,273
Without equity	21	15,004	2,367	11,330	7,170
Without benefits	73	7,618	2,217	3,977	14,523

Note: Costs and benefits are in millions of 1986 dollars.

[a] The reduction in net benefits as compared to the efficient solution.

[b] Sum of HDLDR, SLDR, HSAPP, and SAPP.

for the 31 projects selected to $1.8 billion for 47 projects, however, entails an added efficiency loss of over $5.8 billion (above the actual $3.58 billion social cost), a cost Congress evidently was unwilling to impose. This indicates also that single-minded attempts to reduce federal expenditures can result in significant efficiency costs. Finally, the projects selected outperformed a set of "average" projects in terms of efficiency. If the projects were selected by choosing the "average" project repeatedly, the 45 projects would produce an efficiency loss of nearly $11 billion, with net social benefits totaling only one-half of those actually selected.

I have attempted not only to determine whether there is a pork barrel but also to measure its size. Although the pork barrel does exist, the actual projects selected by Congress far outperform, in efficiency terms, any of the other, admittedly simple, objective functions employed. And while an efficiency cost of almost $3.6 billion is substantial, over 80% of the potential net benefits are realized by the selected projects. Overall, the actual projects selected are surprisingly efficient in light of the mixed "marginal" signals given by the coefficients on the economic variables. This indicates the importance not only of isolating political influence but also of measuring the economic impact of the perceived pork barrel. It should be noted, however, that the efficiency costs presented in Table 8 represent lower bounds of the true efficiency costs, since the opportunity cost of funding one corps project exceeds the net benefits of the most valuable alternative corps project (i.e., there are most likely more efficient ways to spend the money outside the corps's menu of potential projects).

Further evidence of the relative importance of key project characteristics can be understood by examining changes in the model specification and the projects it predicts would have been selected under alternative scenarios.[36] These results are displayed in the lower half of Table 8. The model, with all variables included, predicts (i.e., assigns the highest probability) that 28 projects would be funded with net benefits of $15.5 billion (or a social cost of nearly $3 billion, close to the actual cost of $3.6 billion as measured above). By calculating predicted probabilities without the "economic" variables (project benefits, costs, and benefit/cost ratio), the model predicts a social cost of $14.7 billion for 104 projects. Thus, the simulation suggests that considerable social costs would have been incurred had the decision process ignored the projects' economic characteristics. By omitting all "political" variables from the model, only 11 projects would have been funded, with net benefits of $15.2 billion. This suggests that political factors contribute substantially to inefficiency, and supports a congressional preference for projects spread out over many districts. Finally, estimating predicted probabilities without the "equity" variables also generates substantial economic gains (net benefits would be $11.3 billion). We can infer that decision makers were willing to forego considerable economic benefits in order to satisfy political and redistributional objectives. However, in light of the mixed results concerning equity (states with higher incomes were more likely to receive project funding), only a portion of the net social cost can be attributed to redistributive motives.

The Influence of Cost-Benefit Analysis

We have seen that economic factors enter the decision calculus in unusual ways, some contributing to, and others detracting from, the efficiency goal. While total benefits contribute positively to project selection, so, too, do local costs. The fact that benefits were so influential may be due to the fact that they are another indicator of project size; and since the larger projects in this case were generally more efficient than the smaller ones, the appearance of efficiency was achieved as well. Unexpectedly, however, the benefit/cost ratio is related negatively to project choice. In short, the effects of the project's economic attributes are decidedly mixed with respect to efficiency. An important policy issue concerns the analysis itself: Does the use of cost-benefit analysis influence the project selection process? If so, how?

In order to evaluate its effect, we need to ask what the cost-benefit analysis contributes to the decision-making process that would have been missing in its absence. The corps, administration, and Congress would surely have known, in the absence of any economic analysis, the total cost and the federal cost of every project alternative (though one could argue that the estimates of total and federal costs are improved simply by engaging analysts in preparing cost-benefit analyses, including the recognition of external costs and benefits). Without cost-benefit analysis, decision makers would have been forced to guess the relative benefits of each project, in effect performing their own casual cost-benefit analyses. However, if such information is sufficiently costly to obtain, decision makers may not be able to distinguish between the benefits of one project and another independent of federal costs (which may also represent political benefits). If the latter is true, then the cost-benefit analysis adds two components to the model of the decision-making process: an estimate of the project's benefits and, by extension, its benefit/cost ratio. By calculating predicted probabilities both with and without these variables included in the model, we can measure the independent influence of cost-benefit analysis over project selection.

The results of these newly estimated equations indicate that in each of the three decision processes, the two variables significantly affect the model's results.[37] This implies that the information the cost-benefit analysis imparts has a significant effect on the project funding outcomes. However, given the model's negative benefit/cost ratio coefficient, whether the cost-benefit analysis promotes efficiency or highlights large projects for legislators interested in pork is a separate matter.

The relationship between the CBA and efficient project selection can be addressed by referring to the estimated model to simulate which projects would have been selected in the absence of benefit measures. As reported in the last row of Table 8, using only the economic cost, equity, political, and budgetary criteria (i.e., omitting benefit information), the net benefits of the projects selected would have amounted to $4.0 billion, a net loss of $11.5 billion as compared to the projects predicted with all variables included. Thus, this estimate suggests that cost-benefit analysis had a substantial and positive impact on the efficiency of the corps projects funded by Congress. When compared with the model's estimates omitting the "economic" variables, the results suggest that knowledge of benefits and the benefit/cost ratio (and not the project's total costs) was the primary impetus for efficiency.[38]

A Possible Rationale by the Corps for Cost-Benefit Guidelines

From an organizational perspective, the corps's control over the production of cost-benefit analyses (through its cost-benefit guidelines) can be seen as a way to prevent a disastrous spiral of inflated benefit/cost ratios by each district in order to gain advantage in internal funding decisions. Given a fixed water resource budget, since each of the corps's geographic divisions is vying for a greater share of the available funds, each has an incentive to artifically inflate its own projects' benefit/cost ratio estimates in order to make its projects appear more favorable than those from competing districts. To do so, however, would be to force other districts to mimic the exaggerated net benefit estimates in order to maintain funding parity. This could result in inquiries (e.g., by outside economists) embarrassing the corps over its specious cost-benefit analysis calculations, which, in turn, could result in an overall loss of funding due to public pressure.[39] From the corps's point of view, this perverse incentive, in which each division vying to increase its own funding of projects relative to other corps divisions actually decreases funding for all corps projects, calls for collective action on behalf of the corps to preserve or expand its budget. Indeed, any agency with various offices or districts competing for funds may institute such analytical guidelines not simply to promote "good government" but also to prevent ruinous internal competition through increasingly questionable cost-benefit analyses.

Conclusions

Viewed theoretically, these research findings in effect confirm two quite different theories of federal policy making. The first holds that legislators are driven primarily by their desire for reelection and that among other activities, they will exercise their power to channel funds to their constituents in the form of pork. After redefining pork to account for social welfare criteria, the results show that several House and Senate committees and leadership positions were especially influential in securing corps projects in their districts or states. Viewed from this perspective, the evidence suggests that pork barrel politics remains active and that the nation suffered substantial efficiency losses as a result.

Viewed another way, however, the results indicate strongly that legislators are driven not only by a selfish desire for pork but also by a regard for project attributes commonly thought to reflect the "public interest," namely efficiency and equity. Not only were such attributes statistically significant, but legislators implicitly were willing to trade substantial project benefits on the margin to secure additional equity improvements (note that significant efficiency gains were traded for political benefits as well). In this sense, the "public interest" concerns on the part of Congress and the administration appear to extend beyond mere rhetoric. These empirical results should be accompanied by an acknowledgment of their limitations, noting particularly that the publicly interested behavior may be the result not only of legislative influence but that of the administration or the corps itself. In addition, the relatively small set of projects examined represents only a small part of the business of the committees that oversee the corps.

More importantly, the results of this study cast serious doubt on the methodologies employed in previous studies testing for pork. Given the apparent importance of project-specific economic and equity characteristics in reaching project funding decisions, even at the Army Corps of Engineers, researchers should be skeptical of models that effectively rule out the possibility of social welfare motives on the part of policy makers. Equally important for future research is to distinguish between what I have termed programmatic and allocative pork. While I have found some evidence that specific committees and their constituents benefit from allocative pork, it remains quite possible that the programmatic pork of water resource policy dwarfs the former in significance. A far more general study is necessary to test this proposition. Nonetheless, before concluding that a process is characterized by allocative pork, analysts should be aware that social considerations beyond narrowly defined political self-interest may be important both to agencies and legislators. In addition, our knowledge of public policy would benefit if researchers not only developed a better understanding of the true nature of distributive politics but also provided a measure of the social cost of pork (i.e., How important is it?). In this case, the inefficiency of allocative pork was in some measure offset by the potential inefficiencies of following other simple decision rules that would have proven far more costly.

Appendix

Table A-1. Data Definitions

Type and Variable	Definition
Economic	
BENEFIT	total present value of project benefits ($1,000s)
LOCCOST	total present value of local project costs ($1,000s)
Budgetary	
FEDCOST	total present value of federal project costs ($1,000s)
Political	
HSAPP	project in House Appropriations Subcommittee member's state
HDLDR	project in House leader's district
SLDR	project in Senate leader's state
SAPP	project in Senate Appropriations Subcommittee member's state
HAS	project in House Armed Services Committee member's district
SAS	project in Senate Armed Services Committee member's state
HFAPP	project in House Appropriations Committee member's state
SFAPP	project in Senate Appropriations Committee member's state
NDISTS	number of districts spanned by project
NSTATS	number of states spanned by project
Equity	
RELHVAL	median housing value in district / median housing value in state
INC	state per capita income ($)
POV	persons living below poverty level in state (%)

Sources: Almanac of American Politics, 1985, 1987; *Statistical Abstract of the United States,* 1987.

Table A-2. Summary Statistics of Corps Project Data

Variable	Mean	Minimum	Maximum	Standard Deviation	Units
BENEFIT	247,487	1,887	4,350,000	706,496	$1,000
LOCCOST	22,102	0	295,000	48,584	$1,000
FEDCOST	58,539	655	1,390,000	154,974	$1,000
HSAPP	.26	.00	1.00	0.44	legislators
HDLDR	.02	.00	1.00	0.12	legislators
SLDR	.10	.00	1.00	0.30	legislators
SAPP	.29	.00	3.00	0.68	legislators
HAS	.18	.00	3.00	0.51	legislators
SAS	.45	.00	1.00	0.50	legislators
HFAPP	.29	.00	2.00	0.50	legislators
SFAPP	.68	.00	4.00	0.82	legislators
NDISTS	1.62	1.00	8.00	1.15	districts
NSTATS	1.14	1.00	3.00	0.37	states
RELHVAL	.98	.66	1.94	0.20	proportion
INC	13,562	9,793	18,187	2,062	dollars
POV	12.95	8.00	21.60	3.37	percent

Note: N = 133.

Notes

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1. Because such a project would be attractive to only one legislator (whose district will receive the project), one way to garner support is through logrolling. Another is to change the project scale to extend net benefits to other jurisdictions. The latter approach is, however, in some cases technically impossible. A single sewage treatment plant cannot be located in Maine and Arizona, but legislators in both states can agree to support each other as a form of mutual compensation.

2. By *decreasing social welfare* I refer to the standard Kaldor–Hicks compensation principle, where, in this case, projects could be reallocated so that winners could fully compensate losers and still be better off.

3. Paul Blustein, "U.S. Budget Increasingly Free of Pork-Barrel Spending," *Washington Post*, 21 March 1988.

4. Further assume that the populations within each region are homogeneous and are treated equally with respect to each project choice. This allows us to ignore intraregional distributional effects.

5. I should add that most authors are so vague about what they actually mean by pork that distinguishing between the definitions of cases 5 and 6 is impossible. This is largely a result of ignoring the efficiency consequences of project funding.

6. Congressional appropriations and oversight committees provide one means by which legislators register their preferences for projects beneficial to their constituency. Legislators can to some extent choose committee assignments that reflect constituency interests (Shepsle 1978). Consequently, agriculture committees tend to be dominated by legislators from farming districts, merchant marine and fisheries committees by representatives of coastal districts, and so on. Under this interpretation, legislators select committee assignments (and indirectly, projects) based on reelection goals and so choose committees they feel can provide the greatest net benefits to their districts, which improves their chances for reelection.

7. Of course, legislators may pursue their careers to fulfill other desires as well, such as prestige and power, that may be incorporated into a model of self-interest. For example, Downs notes, "Thus politicians in our model never seek office as a means of carrying out particular policies; their only goal is to reap the rewards of holding office *per se*. They treat policies purely as means to the attainment of their private ends, which they can reach only by being elected" (1957, 28). Kelman argues that "Americans are split on the question of whether congressmen should vote their own best judgment or the opinion of the majority in the district" and cites survey findings that between one-third and one-half of people think legislators should vote their own judgment (1987, 61–62). Kelman presumably views the legislator's best judgment as distinct from inter-

est in being reelected. Kelman notes as further evidence of a lack of congressional interest in pork that the most-sought-after committees are those dealing with broad national issues, which do not normally include public works projects.

8. Some may contend that the more efficient the project, the more benefits the legislator can provide to constituents at a given federal cost. However, the federal cost itself is a benefit to the locality (see Shepsle and Weingast 1981), and the political value of providing a local project is likely to depend as much on the characteristics of the beneficiaries (Are they well organized? Are they currently unemployed?) as on the project's economic efficiency.

9. The revised distributive politics model would assert that agency priorities are a function of P and B, while the public interest model holds that only E and Q affect site selection.

10. Because of long-standing disputes between the Carter and Reagan administrations and the Congress about the size of local cost sharing, the 1986 act was the only major authorization bill for the corps in a decade and the only one expected for some time (Congressional Quarterly 1986, 128).

11. The 1973 *Principles and Standards* established guidelines that the corps (along with other water resource agencies, including the Bureau of Reclamation and the Tennessee Valley Authority) was required to follow in performing its cost-benefit analyses. Including potential projects evaluated under a different set of guidelines prior to 1974 would introduce a bias into the manner in which projects were evaluated by the corps and would have rendered incomparable the cost-benefit analyses used in estimating their effect on project selection.

12. While the list of specific projects being considered is confidential, interviews with corps officials involved in setting funding priorities revealed that these projects were indeed those considered.

13. The total expected cost of the 133 projects in the sample is $10.7 billion, with an estimated federal cost of nearly $7.8 billion—compared with figures of $16.5 billion and $9.5 billion, respectively, for the full 262 projects authorized in the 1986 WRDA (Congressional Quarterly, 1986).

14. Interestingly, the corps recommended appropriating money for only 19 projects; however, since the initial dollar amount of the project requests was not available from the corps, it remains to be seen whether the corps was exercising any degree of fiscal restraint. Since the projects recommended by the corps were large relative to those they rejected (total costs were over twice as large), they may have viewed these as requiring fewer hassles and providing more satisfied customers than if many smaller projects were funded instead. However, Congress does frequently add projects to those included in the administration's budget. Although new starts comprise only a small portion of the corps's annual budget, they are an important indicator of future funding as they reflect current commitments for projects to be completed in future years.

15. Cost-benefit analyses performed for the project sample employed an annual discount rate of 8 5/8%. The real rate of discount used for water resource projects is not determined by the corps itself but rather is mandated by Congress. For years the rate was fixed below 3%. In 1969, however, Congress adopted a flexible system of assigning discount rates: rates would vary depending on the 12-month average of long-term outstanding Treasury debt (15 years or more to maturity), not to exceed changes of 1/4% annually. The discount rate has risen steadily since the late 1960s (see Steinberg 1984), and the corps updates all benefits and costs based on the new discount rate yearly for budgetary purposes.

16. Although they do not ordinarily represent economic benefits, multiplier effects may be important to politicians who wish to claim extra credit for the pork they have brought their constituents. All corps projects are likely to have some multiplier effects producing pecuniary (political) benefits (see Weingast, Shepsle, and Johnsen 1981). The only difference in pecuniary (political) benefits between projects would result from different multiplier effects across districts or regions, which I assume are sufficiently small in this case not to affect project choice.

17. A number of variables that may have been expected to affect funding decisions were surprisingly uninfluential in early model testing. For example, the liberal rating (on economic issues) of the legislators representing the districts or states involved had no effect, just as party affiliation played no apparent role. This serves as evidence against the role of ideology in corps funding decisions (see Kalt and Zupan 1984), though it is only weakly related, since individual votes are not examined (as in their study). In addition, the region of the country to which the project was targeted (divided into the 12 corps divisions) was never influential. This is to be expected, because the corps may, for at least two reasons, try to spread projects geographically: (1) each of the 12 corps divisions may demand its "fair share" of appropriations to maintain organizational unity; and (2) perhaps more importantly, the corps can gain added congressional support by reaching as many districts and states as possible. Other variables that had no effect on funding include the type of project, the state population, and the percentage of the state population living in metropolitan areas. One may have expected that the older projects would be more likely to be funded first, as Arnold (1979) found for water and sewer grants. However, this hypothesis was rejected. Another hypothesis tested was that the corps's appropriations would mirror those of all federal expenditures in the state and thus that the ratio of the state tax burden to federal expenditures

in the state would be negatively related to project funding. This, too, was insignificant in the model. This suggests that there was no apparent trade-off of corps funds with other state funding. Interactive terms, such as the combination of political power and project benefits, were never influential as well. This suggests that while any legislator would naturally prefer a larger project, it is better for a representative to obtain any given project for the constituency than to delay in seeking approval for a large project at the expense of providing smaller ones. Finally, numerous nonlinear variable specifications failed to produce significant results.

18. Only two of the variables change signs from the logit to ordinary least squares specifications—HAS and SFAPP—and at no time are they statistically significant. All other variable signs are stable across the ordinary least squares and logit specifications. Ordinary least squares results are not displayed, since they ordinarily suffer from two problems when estimating discrete qualitative choice models. First, the error terms are heteroscedastic because of the specification of the dependent variable. Second, since there is no assurance that the predicted probabilities lie within the zero-to-one range, model interpretation may be unrealistic (Train 1986). The ordinary least squares regression results are available from me upon request.

19. One criticism of the methodology can be leveled at the relationship between the cost-benefit analysis itself and the decision to approve funding. A skeptic could claim that the corps fudges the analyses that it later wants to promote in the president's budget and therefore that the benefit/cost ratios for favored projects are higher than those of projects the corps, for whatever reasons, does not want to build. As a result, a correlation between benefit-cost ratios and project funding would not necessarily imply an efficient selection process. This seems unlikely, however. First, while the CBAs are prepared at different district offices across the country, each one is reviewed by corps economists in Washington for adherence to the guidelines and for economic content. Thus, although errors in calculation may be made, they can be expected to be consistent across all analyses due to problems in corps guidelines. Second, it seems far more likely to assume that the analysis of the favored project at the district level (where the several alternative means of solving a local problem are considered) might be fudged in order to produce favorable results. However, there is no reason to think that of the *favorable* projects emanating from the district offices (where benefits exceed costs), analyses from one district are altered, consciously or not, any more than those from another district. In other words, even if the benefit/cost ratios are biased for all projects favored at the district level (so that the benefit/cost ratio exceeds unity to assure favorable economic results), the relative benefit-cost ratios among authorized projects are preserved. Third, the sheer length of time between project study and funding is considerable. To systematically plan to alter the analyses of certain projects relative to others would be difficult to accomplish organizationally, to say nothing of the political risk. Projects are sometimes delayed for more than a dozen years before construction; intervening projects the corps may support could be jeopardized by the alteration of previous analyses. It is far more plausible to assume that *all* project analyses are altered in the same manner, even if unwittingly, through established corps procedures and organizational routines, than to believe that certain projects are singled out for special consideration and analysis.

20. Estimating the model without these three variables (project benefits, local costs, and federal costs) yields a chi-squared value $(2 \times (LL_0 - LL_1)$ of 51.40, far exceeding the 99% confidence level of 11.3 with 3 degrees of freedom.

21. Although the Armed Services committees do not have direct jurisdiction over corps policy, interviews with corps officials revealed that because of their military affiliation, they are particularly sensitive to Armed Services committee requests.

22. The negative coefficient for the Senate Appropriations Subcommittee is perhaps best explained by the fact that the Senate appropriations bill rarely includes corps projects, while the House version often includes more than the number requested in the president's budget. For example, in fiscal year 1987 the president's budget request called for 19 new construction starts costing $46.8 million in that year. The House appropriations bill called for 31 projects at a cost of $87.6 million, while the Senate bill included only one project for $17 million. The conference allowance was, as expected, a compromise, with funding for all 31 projects the House wanted but at a cost of only $53.4 million. (It should be emphasized that these regression results indicate only whether the project was funded, not the amount. Therefore, the outcome of the fiscal year 1987 appropriations seems to be skewed heavily in favor of the House even though the annual funding of many of the House projects was reduced considerably). Even though the funding decision was a legislative compromise, the House won initial funding for all of its projects. Since projects started are virtually always (eventually) completed, this can be viewed as a House victory, with the Senate only slowing the pace of construction. Thus, the Senate Appropriations Subcommittee may be seen to exert more influence over the pace of project funding than to determine which projects obtain funding in the first place.

23. In addition to the fact that the magnitude of the state coefficient is considerably larger than the district coefficient in all specifications, the unusual district coefficient may be explained by the fact that two projects, one covering eight districts and the other six, were not funded. Eliminating these proj-

ects from the sample renders the NDISTS coefficient statistically insignificant. Moreover, these results may indicate the relative importance of state delegations in appropriations decisions. Kingdon provides four reasons why legislators cooperate with their state delegations (1973, 85–86): (1) the representative may simply know other state representatives better and may place greater trust in them; (2) a state's interest may be involved, particularly concerning direct economic impacts such as corps projects; (3) maintaining a strong state delegation may aid the legislator's bargaining position in intra-House disputes; (4) it is particularly useful in constituency relations, especially in explaining one's voting record, for "solid" delegation will cause opponents to hesitate in their criticism.

24. Estimating the model without these 10 political variables yields a chi-squared value of 85.04, significant at the 99+% confidence level.

25. The model, estimated without the three equity variables (poverty level, income, and relative housing value), yielded a chi-squared value of 16.58, surpassing the critical value (11.3) at the 99% confidence interval.

26. Under ordinary circumstances, we would not expect the decision patterns of these three institutions to be so uniform, since they respond to different political pressures. However, the log-likelihood functions are nearly identical (−46.371 for Congress versus −46.208 for the president's budget when each equation is estimated separately), and the political variables are as influential in the president's budgetary decisions as they are in congressional funding decisions. The White House appears to be quite receptive to congressional requests for pork. Although the subject is different, this evidence is similar to the findings of Weingast and Moran (1983), who found implicit congressional control over the regulatory decision making of the Federal Trade Commission.

27. This suggests two future lines of research. The first is to examine the *differences* in the public decisions of the administration and Congress. Needed for such a comparison would be a lengthy time series of the different appropriations requests by each decision maker. The "interesting" set of projects would then be, for example, those added or deleted by Congress above those requested in the president's budget. However, because of the continuing arguments over funding shares, very few new start corps construction projects over the past decade have been funded (i.e., the relevant data are not available for the corps). The second line of research would be to model, in a game-theoretic framework, the expectations of each branch in putting forth its proposal in order to determine the relative influence of the administration and Congress. This problem is one of expectations. Presumably, the president's (and the corps's) budget requests are constructed with expectations about congressional funding possibilities. This intricate gaming strategy would have to be modeled carefully, and the relevant information would be extremely difficult to obtain (i.e., access to confidential bargaining strategies). The important matter for researchers is that congressional and administrative budgetary influence may extend well beyond the observed differences in the proposed White House budget and congressional appropriations.

28. This conclusion is reached by comparing the maximum values of the log of the likelihood functions (LL). For the restricted equation the LL was −134.72, while for the unrestricted equation the LL was −126.61. The chi-squared of 16.22 is far less than the critical value of over 44 at the 95% confidence level with 32 degrees of freedom. Therefore, we cannot reject the null hypothesis that the set of coefficients is equal across the three decision types.

29. The chi-squared value is 4.02. Thus, we cannot reject the hypothesis that all benefit/cost coefficients are equal at even the 90% level of confidence. In this regard, what appears to be most important is that the benefit/cost ratio is greater than unity. In other words, the marginal contribution to the probability of project acceptance approaches zero once a project has cleared the benefit/cost "hurdle."

30. The elasticity estimates represent the percentage change in the probability of funding (at each of the three levels) induced by a 1% increase in the independent variable displayed, evaluated at the mean of all independent variables. For example, a 1% increase in project benefits (from roughly $247 million to $249.5 million) would increase the probability of project funding by .51%. Although the elasticities for the political variables are displayed, their interpretation is not particularly relevant. Since these variables are discrete integer quantities, a 1% increase is not meaningful. The more realistic interpretation included in the next section displays the marginal rates of substitution between variables.

31. This is simply the ratio of the two coefficients: for the constrained decision process, the coefficient on total costs (LOCCOST + FEDCOST) was −1.13, compared to 1.46 for benefits, a ratio of .77.

32. These figures are constructed by holding all other independent variables constant at their means and manipulating only the value of the variable in question. In each case the horizontal axis represents the total project benefits in millions of dollars, while the vertical axis represents the probability of funding. The average project benefit is "247" along the x-axis, illustrating in most every case that high benefit levels are necessary to raise the probability of funding above 50%. Slightly different figures would result from graphing the effects of, say, political variables on the probability of congressional funding alone. As such, these figures represent "averaged" coefficients. These figures are for the model where all coefficient estimates are constrained to be equal (see Table 4).

33. Indeed, replacing the poverty level variable with a binary variable coded as 1 if the poverty level is one standard deviation over the mean (16.3%) produces logit results that are virtually indistinguishable from the results shown in Table 4. The similarity of the likelihood ratios (−134.20 vs. −134.72) indicates that the particularly depressed regions are those that explain most of the variation in funding decisions ascribed to poverty level differences.

34. Other ways of spending the $3.66 billion obviously may be superior to funding water resource projects to many observers, and the choice of the actual amount funded is admittedly arbitrary. However, I take the $3.66 billion as a statement of the political will of Congress to fund new corps water resource projects. As a result, the relative efficiency of all the alternative projects considered may be overstated. However, we are concerned here with the relative efficiency of one objective versus another, not the absolute impact. Thus, the use of the corps's cost-benefit analyses relies not on their absolute, but rather their relative, accuracy.

35. These include (1) that all costs are borne by the government (which is reasonable at the corps), (2) that projects are mutually exclusive, and (3) that a budget constraint exists (which is undeniable).

36. The following results are derived by estimating the model's predicted probability of project acceptance without certain variables included (e.g., political factors). By then ranking the 133 projects in terms of their predicted probability of acceptance and funding them until the fixed social costs are expended, I derive a new choice set of projects indicating which projects would have been hypothetically funded in the absence of certain elements of the decision-making process.

37. A model including benefits, total costs, benefits/costs, federal costs, and all other variables in Table 4 was estimated, generating a log of the likelihood function value of −131.30. The same model, estimated without benefits and benefits/costs, produced a value of −147.86. The chi-squared value of 33.12 is significant at the 99% level of confidence, implying that we can confidently reject the hypothesis that the benefits add nothing to predicting funding outcomes. This outcome holds for each equation estimated separately, as well, at the 99%, 95%, and 99% levels for Congress, the administration, and the corps, respectively.

38. Including project costs adds only 4.7% to net social benefits, from $3.8 billion to $3.977 billion, while including project benefits and the benefit/cost ratio raises net benefits from $3.977 to $15.5 billion, an increase of 290%.

39. One could argue that this has already happened. Following studies by Haveman (1965, 1972) on the corps as well as other studies critical of its cost-benefit procedures (e.g., Drew 1970), the corps instituted much more exacting procedures for computing costs and benefits. Though far from perfect, the procedures are a vast improvement over the earlier, overly vague, guidelines.

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CONSTITUENCY OPINION AND CONGRESSIONAL POLICY MAKING: THE REAGAN DEFENSE BUILDUP

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Representatives' votes on a series of defense budget roll calls in the first year of the Reagan administration's Pentagon buildup are related to constituency opinions on defense spending during the 1980 election campaign. The strong aggregate constituency demand for increased defense spending in 1980 is estimated to have added almost $17 billion (about 10%) to the total fiscal year 1982 Pentagon appropriation. The impact of constituency opinion was largely independent of specific political circumstances: differential responsiveness in districts with partisan turnover, intense district-level competition, and strong presidential coattails together accounted for less than $1 billion in additional appropriations, with the remaining $16 billion attributable to across-the-board responsiveness by even the most safely incumbent representatives.

The appeal of representative democracy hinges on the responsiveness of elected politicians to the preferences and interests of their constituents. Given the theoretical and practical significance of the connection between constituents' opinions and their representatives' legislative activities, it is remarkable that the large and in some ways quite sophisticated empirical literature addressing this connection includes so few direct estimates of the relationship between constituency opinion and specific legislative outcomes.[1] My aim here is to provide one such estimate.

The policy issue I have chosen to examine is the level of total Pentagon spending in the first (FY 1982) defense appropriations bill of the Reagan administration. That bill, the first in a series of annual appropriations resulting in a 40% real increase in defense spending during Reagan's first five years in office, is of obvious political significance. But the issue of defense spending is also especially interesting from a broader theoretical perspective because the strong public demand for a defense spending increase at the beginning of the Reagan era provides unusual analytical leverage for assessing the potential impact of public opinion on the making of public policy.

I shall describe some alternative mechanisms of congressional responsiveness to constituency opinion; briefly characterize the politics of defense at the beginning of the Reagan era; present my analysis of representatives' preferences regarding FY 1982 defense appropriations, relating those preferences to constituency opinions on the defense spending issue and to other political characteristics of the representatives and their districts; extend the analysis of FY 1982 defense appropriations from the individual to the aggregate level in an attempt to estimate the impact of congressional responsiveness on the total level of Pentagon appropriations; and finally, touch upon some of the implications of my analysis for the study of linkages between public opinion and public policy.

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Parties, Elections, and Congressional Responsiveness

How does constituency opinion get translated into public policy? The emphasis in the modern tradition of democratic thought variously encompassed by "rational choice," "responsible party," and "realist" theories has been upon the importance of electoral competition for inducing political responsiveness, either through actual partisan turnover or through anticipation by incumbents of constituents' policy demands (Downs 1957; Schattschneider 1942; Schumpeter 1950). But for observers of the contemporary U.S. Congress, both the steady pull of prospective electoral competition and the occasional push of widespread partisan turnover have seemed tenuous mechanisms for ensuring democratic accountability. Increasingly, incumbent representatives have been returned to office reliably, overwhelmingly, and independently of broad national political currents.[2] Presidential landslides have been common enough; but instead of signaling major and enduring "party realignments," those shifts at the presidential level have increasingly left Congress essentially untouched.[3]

The significance of this historical trend has been stressed most forcefully by political historians raised on the major party realignments—and subsequent policy shifts—of earlier eras. Thus, Brady (1988, 181) argued that:

> given the noncompetitive nature of contemporary House elections, two conditions of responsible government are difficult to meet. Presidents will not carry majorities into office with them, and there will be little alternation of or undivided control of the government. Under these conditions American government will continue to be characterized by drift rather than mastery, and by fragmentation rather than coherence.

Of the Reagan era in particular, Brady (p. 164) argued that "there was not enough turnover to create the conditions (committee changes and other shifts discussed in chapters 2-4) to change policy in a significant or permanent manner."

If Brady is right—if large-scale turnover in Congress is a necessary precondition for significant policy change—then the prospects for such change do appear bleak.[4] It has become increasingly difficult to envision an electoral unheaval of sufficient magnitude to produce congressional turnover on the scale of the classic "critical elections" of earlier eras.

On the other hand, perhaps we should expect incumbent representatives, both individually and collectively, to respond to significant changes in constituency opinion even when they face little real danger of being voted out of office. Within the "realist" model such behavior could be rationalized as evidence of extreme risk aversion: if representatives care *only* about reelection, they may be happy to bend in any breeze rather than risk a revolt in the district, however unlikely it may be. More realistically, even representatives who realize that their seats are quite safe may feel bound as a matter of duty to give some independent weight to their constituents' more fervent opinions when those opinions conflict with the representatives' own. Finally, and perhaps most importantly, the same objective events that influence constituents' opinions may also influence representatives' own opinions about good policy, producing, if not responsiveness, at least congruence between the views of representatives and the views of their constituents.

My argument here is that for one or more of these reasons, even securely incumbent representatives did vigorously represent their constituents' desires for increases in defense spending in 1981. Thus, it appears that important policy changes can and do occur even in the absence of significant congressional turnover. Under current circumstances, elections seem like blunt instruments at best for ensuring congressional responsiveness; but Congress seems responsive nevertheless, and that fact is sufficiently important and intriguing to warrant sustained attention.

The Politics of Defense, 1981

"The *prevailing* consensus," one recent scholarly study reported, "is that the public possesses little information and only few, ill-formed attitudes" about defense and foreign affairs and thus that "such concerns are not terribly consequential in the voting booth" (Aldrich, Sullivan, and Borgida 1989, 125). That consensus, however accurate it may once have been, appears since the late 1970s to have become increasingly untenable. In 1980 about one third of the public mentioned defense and foreign policy issues as the nation's most important problem, and more people correctly characterized the relative positions of Reagan and Carter on the issue of defense spending than on any other issue (Aldrich, Sullivan, and Borgida 1989, 130, 137).

Two events made the issue of national defense especially salient in the 1980 campaign. First, in November 1979 Iranian militants seized the U.S. Embassy in Tehran. The U.S. occupants of the embassy compound were paraded across the world's television screens as hostages, while President Carter was taunted and burned in effigy; five months later a hostage rescue effort collapsed when helicopters needed to ferry a U.S. strike force to Tehran broke down in the Iranian desert, and the hostage crisis dragged on until the day Carter left office. Second, in December 1979 the Soviet Union invaded Afghanistan. As it turned out, that invasion represented the beginning of a long, costly, and (eventually) formally repudiated Soviet attempt to prop up a failing client regime; but at the time—especially in the aftermath of previous communist thrusts in Cuba, Nicaragua, Angola, and Ethiopia—it seemed to many in the United States to reflect a significant deterioration in the United States' global strategic position.

Ronald Reagan and the Republican party seized upon events in Iran and Afghanistan to help crystallize widespread disquiet about the United States' standing in the world, and to turn that disquiet into a Republican campaign issue. The tone of the indictment is suggested by passages on defense policy from the 1980 Republican Party Platform (Congressional Quarterly 1980, 75B–76B):

> The [Carter] Administration's neglect of America's defense posture in the face of overwhelming evidence of a threatening military buildup is without parallel since the 1930s. The scope and magnitude of the growth of Soviet military power threatens American interests at every level, from the nuclear threat to our survival, to our ability to protect the lives and property of American citizens abroad. . . . Despite the growing sentiment for a stronger defense, candidate Carter ran on a promise of massive cuts in U.S. defense spending, one promise he has kept. . . . His tough speeches before military audiences cannot hide his continuing opposition to Congressional defense increases. . . . We have depleted our capital and must now devote the resources essential to catching up.

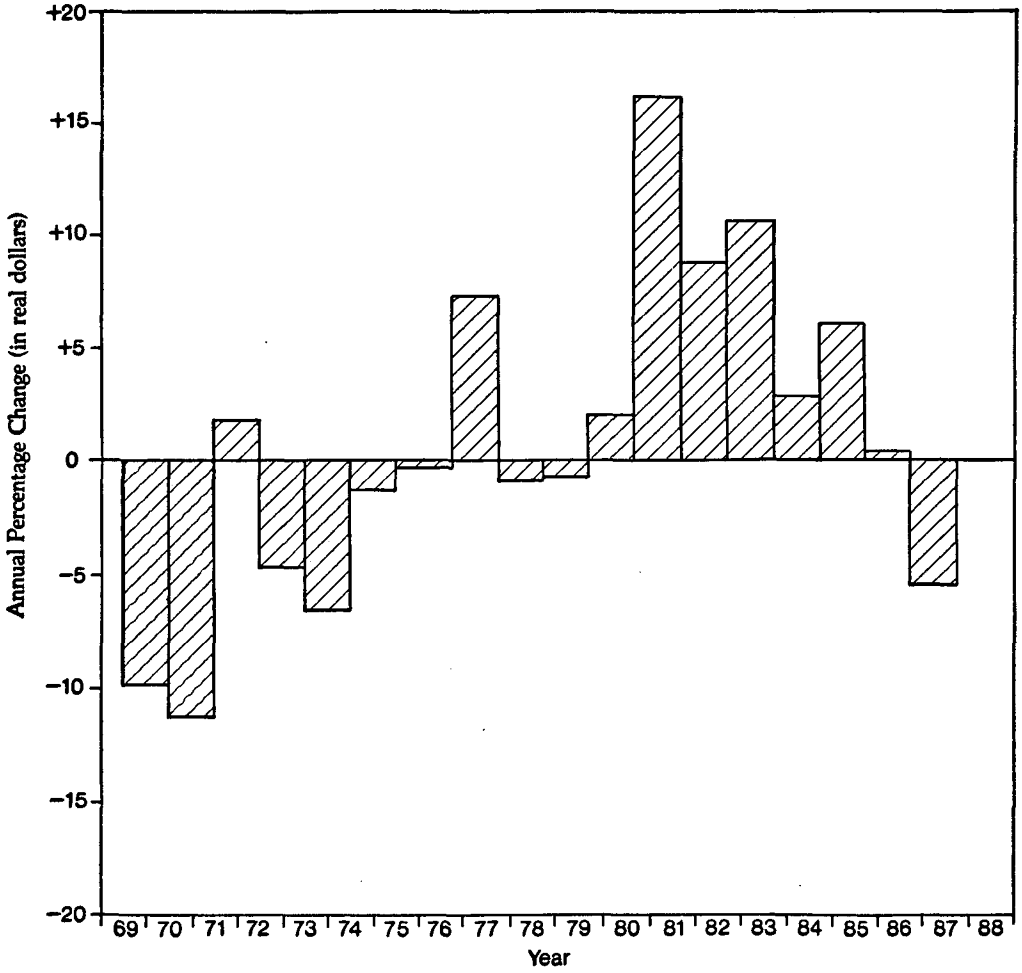
Taken literally, much of this Republican indictment was unsupported by the facts. As Figure 1 indicates, Carter's first three defense appropriations essentially kept pace with inflation, even after some whittling down by Congress. Increasingly, however, Carter's moderate approach put him behind the curve of public (and congressional) opinion. As Figure 2 shows, a small public plurality favoring defense spending increases had emerged even before the events in Iran and Afghanistan. In the first half of 1980 that plurality grew so rapidly that throughout the campaign season, the fraction of the public favoring defense spending increases outnumbered the fraction favoring decreases by about 40 percentage points.[5]

Carter proposed a real increase in defense appropriations of almost 5% for

FY 1981 and significant additional increases for FY 1982, but the Republican platform was not alone in criticizing these proposals as too little and too late. Six weeks before Election Day the House voted a real increase almost twice as large as the one Carter proposed for FY 1981, and candidate Reagan promised more to come.

The strong public demand for a defense buildup (and the clear perceived differences between the party's presidential candidates on the issue) made defense policy a significant issue in the 1980 campaign. One analysis intended to measure the net effects of party identification, ideology, and a variety of personal assessments, performance evaluations, and issue preferences in the 1980 presidential election suggested that the defense spending issue contributed 7.6 percentge points —much more than the 3.2 percentage points contributed by a whole series of five domestic issues—to the Reagan mar-

Figure 1. Pentagon Appropriations
(including supplemental appropriations)



gin of victory (Miller and Shanks 1982, 349).

Swept into office by a strongly pro-defense electorate, Reagan redeemed his campaign pledges by proposing a significant acceleration of the U.S. defense buildup. A supplemental appropriation redoubled the real increase for FY 1981, and in March the new administration requested $222 billion in new budgetary authority for Pentagon programs in FY 1982, $26 billion more than Carter had requested in January.[6] The new Republican-controlled Senate was a more-than-willing partner in the Reagan defense initiative, eventually passing a FY 1982 defense appropriation bill that exceeded the president's own revised request. Thus, the House of Representatives—especially a bipartisan group of pivotal moderates in the House—found itself facing from the Right a major and popular policy initia-

Figure 2. Public Opinion Toward Defense Spending

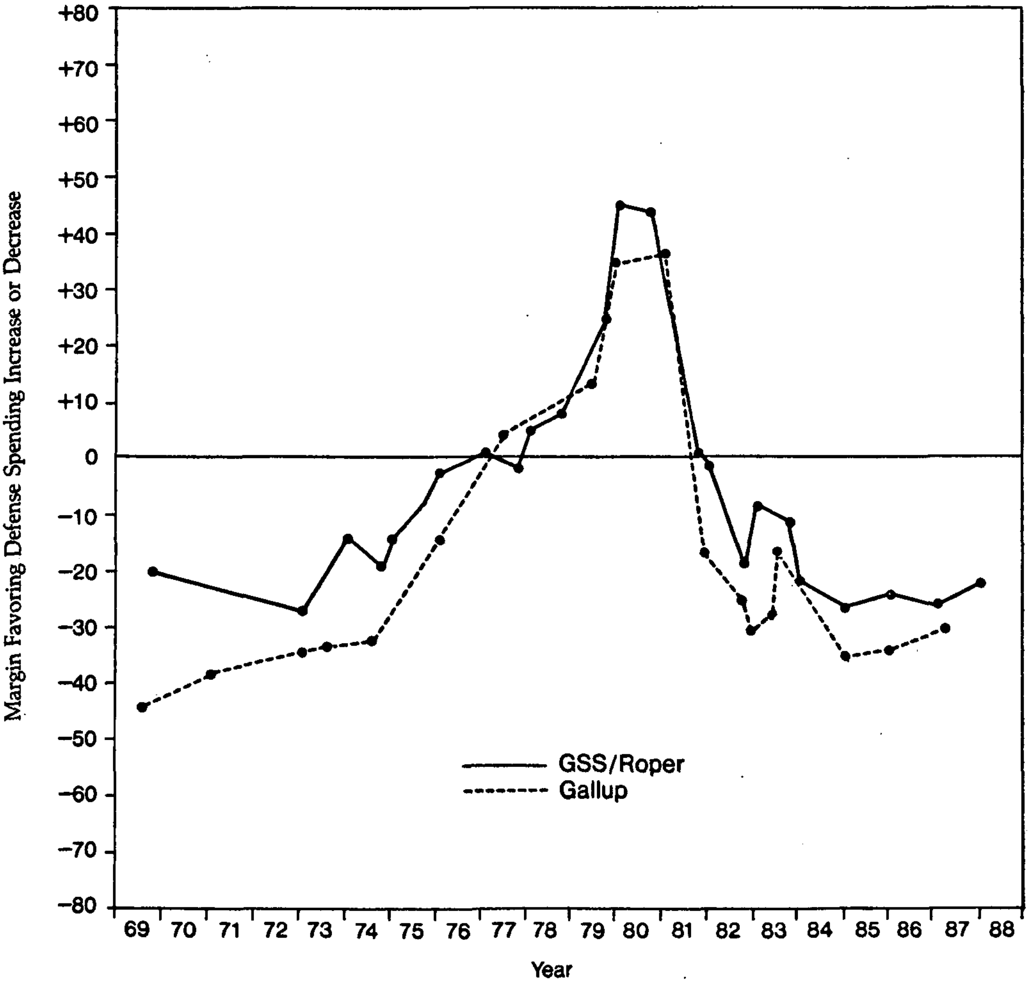
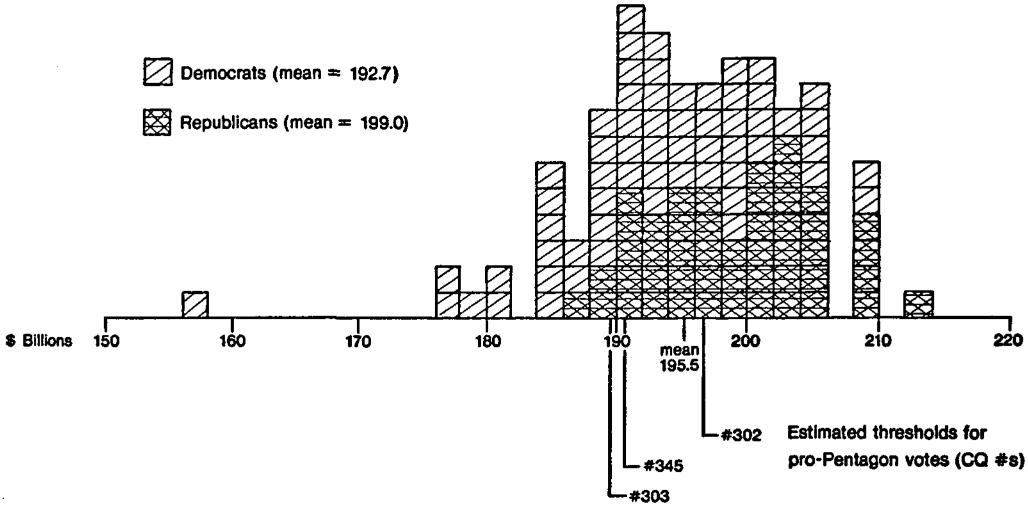


Figure 3. Imputed Preferred Levels of FY 1982 Defense Appropriations
(for 108 representatives in 1980 NES sample)



tive and from the Left a long series of amendments intended to block or roll back that initiative.[7] The present analysis examines how individual representatives and the House as a whole responded to those political forces.

Sources of Congressional Support for Defense Appropriations

What factors account for representatives' positions on the issue of defense budget policy during the crucial first year of the Reagan Pentagon buildup? Analyses of legislative behavior, both quantitative and qualitative, have often been hamstrung by the difficulty of imputing policy preferences to legislators. Here I use a technique developed by Krehbiel and Rivers (1988) to estimate representatives' positions on an underlying policy dimension by relating characteristics of the representatives and their districts to observed behavior on a sequence of related roll call votes.

The sequence of votes I analyze in detail covers three stages of the FY 1982 defense appropriations process: an amendment to reduce the amount of money appropriated for weapons procurement and research and development, the appropriations bill itself, and the conference report reconciling the House and Senate appropriations figures. By observing various combinations of positions on these three votes (and relating them to observed characteristics of the representatives and their districts), it is possible to estimate the underlying preferences of individual representatives regarding overall levels of defense spending.[8]

The resulting estimated FY 1982 defense appropriations preferences of the 108 representatives in the National Election Study (NES) sample are shown in Figure 3.[9] The estimated mean preference is for $195.5 billion in FY 1982 defense appropriations, about $4 billion less than was actually appropriated. (Recall that the final appropriation represented a compromise with the Senate's higher figure.) Except for a single anti-Pentagon outlier, the range of estimated preferences is from

Table 1. Sources of Support for Pentagon Appropriations

Variable	Units	Estimated Effects (Billions of Dollars)
Intercept	—	183.96 (13.40)
Constituency opinion	NES 7-point scale	12.87 (5.82)
Constituency × competitiveness	NES scale × loser's % vote	.0112 (.0497)
Tax burden	$1,000s per capita	-4.14 (3.72)
Pentagon outlays	$1,000s per capita	7.70 (3.68)
Partisanship	Republican = 1, Democrat = 0	3.87 (1.79)
Presidential influence	vote difference in 100,000s	4.69 (3.45)

Note: Standard errors of estimated effects are in parentheses. Based on probit analysis "including competitiveness" reported in Table A-2 in the Appendix.

about $175 billion (a real decrease of almost 5% from the supplemented FY 1981 appropriation) to about $215 billion (a real increase of more than 15% over the supplemented FY 1981 appropriation).

The parameter estimates in Table 1 relate the imputed appropriations preferences shown in Figure 3 to salient characteristics of the representatives and their districts.[10] The characteristics whose effects are estimated in Table 1 are of three general sorts: constituency opinion, economic interests, and partisan political factors (including the representatives' own partisanship and presidential influence).

Constituency Opinion

My main interest here is in the relationship between constituency opinion and representatives' legislative behavior. One of my reasons for focusing on the specific issue of defense spending is that the 1980 NES survey included an item directly tapping constituents' opinions on that issue: "Some people believe that we should spend much less money for defense. Others feel that defense spending should be greatly increased. Where would you place yourself on this scale, or haven't you thought much about this?" Respondents were offered a seven-point scale with the endpoints labeled *greatly decrease* and *greatly increase.* In my recoding of the raw data these endpoints correspond to scores of −3 and 3, respectively. In each congressional district my measure of constituency opinion is the mean score (based on an average of 19 individual responses) on this seven-point scale.[11] It is a testament to the force of prodefense sentiment in the nation as a whole in 1980 that of the 108 congressional districts included in the NES survey, 107 had mean constituency opinions favoring defense spending increases.[12]

The estimated impact of that prodefense sentiment on representatives' appropriations preferences in Table 1 is quite striking. Every one-point increase or decrease in mean constituency opinion on the NES seven-point scale produced an estimated increase or decrease in congressional defense appropriations preferences of almost $13 billion. Given the observed range of district means—3.5 points on the NES seven-point scale—this estimate suggests that individual representatives' appropriations preferences varied by as much as $45 billion (over a total appropriation of less than $200 billion) due to variations in constituency preferences. By the same estimate, a difference of a bit less than half a point on the seven-point scale (one standard deviation in the distribution of district means) would be sufficient to produce a difference of $6.3 billion in appropriations preferences—the difference between the preferences of an average Democrat and an average Republican in Figure 3.

Table 1 also includes an estimate of how the impact of constituency opinion varied with the electoral competitiveness of specific congressional districts. The idea is that representatives elected by relatively narrow margins might be especially sensitive to the policy demands of their constituents. Some evidence of differential responsiveness consistent with that hypothesis does appear, but it is too slight and too imprecise to amount to much. A representative elected by the narrowest possible margin would, by this estimate, be less than 5% more responsive to constituency opinion than one elected without any opposition at all.

Economic Interests

Another obvious source of district-level variation in support for defense spending is district-level variation in the economic costs and benefits of various Pentagon activities. Other things being equal, we expect representatives—in the realm of defense spending as elsewhere—to support programs that bring their constituents contracts and salaries and to oppose programs that cost their constituents tax dollars, even when those costs and benefits are not entirely reflected in aggregate constituency opinion.

While it would obviously be very helpful to have detailed data on the economic implications of every roll call for every congressional district, readily available data are much rougher. Here, I measure each district's stake in the Pentagon budget as a whole by including among the explanatory variables a single measure of costs (annual per capita federal tax payments) and a single measure of benefits (annual per capita outlays by the Department of Defense). The roughness of the measures is exacerbated by the fact that the relevant data are available not at the level of congressional districts but at the level of states.

In spite of these difficulties, both measures have plausible estimated impacts in the analysis of defense appropriation preferences reported in Table 1. A thousand-dollar per capita difference in annual Defense Department outlays corresponds to a $7.70-billion difference in imputed congressional preferences for FY 1982 defense appropriations, while a thousand-dollar per capita difference in tax burdens corresponds to a −$4.14 billion difference in preferred defense appropriations. These effects are especially impressive if one bears in mind that economic interests also have a substantial indirect effect on congressional preferences through their effect on constituency opinion.[13]

Partisanship

Does it matter that the partisan division of congressional seats is considerably more stable from one election to the next than it was before 1950? Net partisan turnover is significant to the extent that Democrats and Republicans behave differently in Congress. But it is important to avoid confusing the net effect of partisanship per se with the total observed difference between Democratic and Republican representatives, since much of the total difference may be attributable not to the partisanship of Democratic and Republican *representatives* but to the distinctive characteristics of Democratic and Republican *districts.* Here I attempt to characterize the real political impact of partisan turnover by estimating the effect of a representative's party affiliation on his preferred level of defense appropriations after statistically controlling for relevant characteristics of his constituency.

The estimate in Table 1 suggests that, other things being equal, Republican representatives wanted about $3.9 billion more than their Democratic counterparts in FY 1982 Pentagon appropriations. This estimate of the net effect of partisanship represents only about three-fifths of the observed $6.3 billion partisan difference in appropriations preferences in Figure 3; the remainder of the observed aggregate difference is attributable to the fact that Republicans tended disproportionately to represent pro-Pentagon districts and Democrats tended disproportionately to represent anti-Pentagon districts.

Presidential Influence

Students of the presidency at least since Neustadt (1960) have observed that presidents pushing for significant policy changes can succeed only if they find the political means necessary to influence the behavior of other actors. Furthermore, they have noticed that "election outcomes obviously bear upon [the president's] chances to obtain his policy ends by other means" (Neustadt 1960, 91). For a representative, the most relevant election outcomes are presumably those in his own district; thus, the president's power to persuade should vary directly with presidential support in the representative's district and inversely with the political insulation provided by the representative's own district-level support. Thus, it seems reasonable here to measure Reagan's (potential) influence over (potentially) unwilling representatives by the extent to which his 1980 vote total in each district led or trailed the congressional winner's own vote total.

It is an interesting comment on the relative electoral security of most representatives that Reagan's victory left him well behind the winning congressional candidate—Democratic or Republican—in most districts.[14] The estimated effect in Table 1 suggests that on average, the one representative in five who trailed Reagan in his own district in 1980 was willing as a result to spend almost $4 billion more on defense in 1981 than was the one in five who outpolled Reagan by more than 50 thousand votes.

The FY 1982 Defense Buildup: A Political Accounting

I have examined the determinants of individual representatives' preferred levels of Pentagon appropriations. I now turn from an individual to a collective perspective: my aim here is to analyze the implications of the results in Table 1 for the aggregate level of Pentagon appropriations for FY 1982. The relevant calculations are summarized in Table 2.

My starting point in accounting for the level of Pentagon appropriations in FY 1982 is the level of appropriations in FY 1981. Congress appropriated $171.31 billion for the Department of Defense in FY 1981. Including an inflation adjustment of $12.11 billion,[15] the corresponding appropriations level for FY 1982 would amount to about $183.42 billion. The difference between this baseline spending level and the actual FY 1982 Pentagon appropriation, $199.69 billion, reflects the real increase in Pentagon spending resulting from the FY 1982 appropriations process.

One difficulty in estimating the aggregate impact of political factors on Penta-

Table 2. FY 1982 Pentagon Appropriations: A Political Accounting

	Billions of Dollars
Total FY 1981 appropriation (including 11.57 supplemental)	171.31
Inflation adjustment (7.07% 1982 government purchases price inflation)	12.11
Constituency opinion	15.89
Constituency × competitiveness interaction	.38
Partisan replacement	.31
Presidential influence	.18
Unaccounted for	-.48
First FY 1982 Pentagon appropriation	199.69

Note: Appropriations exclude military construction (covered by a separate bill) and defense-related activities outside the Department of Defense.

Sources: For appropriations, *Congressional Quarterly* 1980, 186; *Congressional Quarterly* 1981, 321, 353. For inflation adjustment, *Statistical Abstract of the United States 1987,* 456. For political effects, estimates in Table 1.

gon outlays is to establish appropriate baselines from which to measure the effects of those factors. Constituency opinion has a natural baseline at the point where aggregate national opinion favors neither an increase nor a decrease in the current level of defense spending. Thus, I estimate the total impact of constituency demand by multiplying the observed mean national position on the NES defense spending scale (1.235) by the estimated effect of constituency opinion on representatives' appropriations preferences in Table 1 ($12.87 billion) to produce an estimated aggregate impact of almost $16 billion.

Now, $16 billion is a considerable chunk of money, even for the Pentagon. But the apparent effect of constituency opinion in Table 2 is still more impressive when we recognize that it significantly understates the actual magnitude of congressional responsiveness to constituency opinion in at least four ways. First, as I have indicated, Congress had already responded to constituency demands for Pentagon spending by voting huge increases in defense appropriations for FY 1981, once in the last year of the Carter administration and again in a supplemental appropriation after Reagan assumed office; the FY 1982 increases attributed to constituency opinion here are in addition to those in the FY 1981 budget. Second, as Figure 2 makes clear, much of the public enthusiasm for defense spending increases measured by the NES survey in late 1980 had already waned by late 1981, when representatives were casting the roll call votes analyzed here; to the extent that representatives were cognizant of current constituency opinion on the issue, they were actually responding to a considerably weaker aggregate public demand for defense spending increases than the one portrayed in the 1980 NES data. Third, representatives in late 1981 were responding not only to a public demand for defense spending increases but also to simultaneous public demands for social programs, tax reduction, and fiscal responsibility; the contradiction inherent in these simultaneous demands manifestly limited the ability of Congress to respond to each of them separately.[16] Finally, the increase in appropriations for FY 1982 shown in Table 2 understates the long-run commitment to increased defense spending entailed in the FY 1982 Pentagon appropriations votes, since the budget increases approved for FY 1982 were disproportionately concentrated in big new procurement programs whose total cost would be spread out over several years.[17]

It is impossible to estimate precisely the magnitude of the underestimation resulting from these various factors. However, it does seem clear that the increase in appropriations attributed to constituency opinion in Table 2 should be thought of not as a comprehensive estimate of the total effect of the public demand for defense spending increases documented in

Figure 2, but as a lower bound upon such an estimate. The magnitude of the effect is, by any standard, substantial.

As it turns out, this undifferentiated effect of constituency preferences accounts for the vast bulk of the actual increase in Pentagon outlays. Multiplying the observed national mean defense spending preference by the observed mean challenger's vote percentage (27.8) and the estimated interactive effect in Table 1 ($.0112 billion) produces an estimated aggregate impact of district competitiveness of only $ .38 billion. Similarly, multiplying the observed 1980 net partisan turnover of 7.9% in the House of Representatives by the estimated effect of partisanship in Table 1 ($3.87 billion) produces an estimated aggregate impact of partisan replacement of only $ .31 billion.

For presidential influence the appropriate baseline is less clear. Having measured Reagan's potential influence by his vote margin relative to the winning congressional candidate in each district in 1980, I somewhat arbitrarily adopt as a baseline for comparison the corresponding margin for Gerald Ford in 1976. Reagan in 1980 ran an average of 3,833 votes closer to the winning congressional candidate in each district than Ford had done in 1976; multiplying this average increment in (potential) influence by the estimated effect of presidential influence in Table 1 ($4.69 billion per hundred thousand votes) produces an estimated aggregate impact of presidential influence of only $.18 billion.

Taken together, the four political effects represented in Table 2 account for a real increase in FY 1982 Pentagon appropriations of $16.76 billion. This figure matches quite closely the actual real increase of $16.27 billion in FY 1982 appropriations. The correspondence must be regarded as at least partly coincidental, since the calculations in Table 2 ignore both errors of model specification and estimation[18] and other influences on congressional behavior; nevertheless, the coincidence should give some pause to those who would prefer on ideological grounds to view defense spending in general, and the Reagan defense buildup in particular, as fundamentally undemocratic phenomena.

Conclusion

The analysis presented suggests that public opinion was a powerful force for policy change in the realm of defense spending in the first year of the Reagan administration. Moreover, the impact of constituency opinion appears to have been remarkably broad-based, influencing all sorts of representatives across a wide spectrum of specific defense spending issues.[19]

Whether constituency opinion has a similar impact on congressional policy making in other issue areas and under other political circumstances is, of course, an open question. For all its volume and apparent sophistication, the empirical literature on representation does little to help answer that question.[20] Certainly, aggregate changes in public opinion of the magnitude and salience observed on the defense spending issue in the late 1970s are rare.[21] But at this point we simply know too little to be able to guess with any confidence whether the effect of less dramatic constituency demands for policy change are likely to be proportional to their magnitude, their intensity, their specific source, or some complicated combination of all three.

To the extent that the analysis reported here does turn out to be representative of other times and other issues, it suggests, contrary to some accounts, that Congress can produce substantial policy changes even in the absence of significant turnover. Given the fickle and fragmentary nature of public opinion on many issues, these policy changes may seldom be sweeping or sustained; certainly the public and Congress alike cooled markedly in their enthusiasm for defense spending even by the end of 1981. But then, a policy realignment of the classic sort may not be so desirable after all if it merely serves to preserve and exaggerate a temporary public enthusiasm in the legislature.

The analysis presented here also suggests (though it does not conclusively demonstrate) that congressional responsiveness depends on a more complex mixture of motives than is typically assumed in the scholarly literature. The allure of a simple assumption about motivation—that elected officials respond to their constituents in order to maximize their own chances for reelection—is obvious. But the practical limitations of that assumption should also be obvious when, as here, representatives who win with 100% of the vote appear to be about as responsive to constituency opinion as those who win with 51% of the vote. Either subjective safeness bears no relationship to objective safeness (e.g., because representatives are risk-averse to the point of total spinelessness) or (as seems more likely) the reelection motive must be thought of as only one—albeit an important—element in a broader array of congressional goals.[22]

Finally, the pattern of results reported here cries out for further exploration of the actual political processes relating constituency opinion and legislative behavior. Does representation occur because representatives really do know and heed average constituents' views about specific policies, or because those average constituents' views happen to mirror more influential views (of the Washington policy community, district elites, or the representative him- or herself)? Is the stability of voting alignments across a wide range of detailed issues primarily a Washington phenomenon (reflecting the mutual influence of working networks of like-minded representatives), or primarily a district phenomenon (reflecting independent choices by individual representatives similarly estimating the latent opinions of their uninformed constituents)? The observational literature on Congress and representatives (e.g., Fenno 1978; Kingdon 1981) provides one valuable set of insights into questions like these. Nevertheless, as much remains to be learned about *how* political responsiveness works as about *how well* it works.

Appendix

The Sample and the Data

The 108 representatives included in my analysis are those whose districts were included in the sampling frame for the 1980 American National Election Study.[23] The explanatory variables appearing in the analysis (with descriptive statistics for the 108 districts in the NES sample follow.

Black. District percentage black, 1980 census (Min = 0; Max = 87; Mean = 13.4; SD = 17.1).

Competitiveness. Loser's share of district two-party congressional vote (%) in 1980 election (Min = 0; Max = 50; Mean = 27.8; SD = 15.0).

Constituency Opinion. Estimated district mean on NES seven-point defense spending scale: −3 = greatly decrease; 0 = no change; 3 = greatly increase (Min = −1.25; Max = 2.25; Mean = 1.205; SD = .469). Calculated from 1980 NES.

Partisanship. Indicator variable for districts with Republican representatives in 1981 (Mean = .444).

Pentagon Outlays. Defense Department expenditures per capita (in thousands of dollars), 1979, by state (Min = .109; Max = 1.288; Mean = .454; SD = .259).

Population Change. Percentage change in district population, 1970–1980 (Min = −.24; Max = 94; Mean = 12.67; SD = 20.86).

Presidential Influence. Number of votes (in hundred thousands) by which Reagan led (trailed) congressional winner in 1980 election (Min = −1.174; Max = .244; Mean = −.269; SD = .294).

South. Indicator variable for districts in Alabama, Arkansas, Florida, Georgia, Louisiana, Mississippi, North Carolina, South Carolina, Tennessee, Texas, and Virginia (Mean = .241).

Tax Burden. Federal tax revenues per capita (in thousands of dollars), 1979, by state (Min = 1.288; Max = 2.655; Mean = 2.012; SD = .295).

West. Indicator variable for districts in Arizona, California, Colorado, Oregon, Utah, Washington, and Wyoming (Mean = .176).

All of the data except those for constituency opinion are from Barone and Ujifusa 1981. Mean constituency opinions are based on between 6 and 46 responses per district on the NES defense spending question (excluding missing data and respondents with no opinion on the question); the standard errors of the resulting estimated constituency means range from .166 to .773 on the NES seven-point scale.

Because the estimated constituency means contain substantial sampling error, ordinary regression analyses employing constituency opinion as an explanatory variable will produce inconsistent parameter estimates. The correction employed here is based on a purging regression relating the mean constituency opinion in each district to a variety of other (by assumption exogenous) district characteristics. Because the magnitude of the sampling error varies across districts, each district is weighted in the purging regression by the reciprocal of the standard error of the estimated district mean constituency opinion. (Thus, districts with more survey respondents get more weight in the purging regression.)

The results of the purging regression are reported in Table A-1. The regression coefficients make good sense (except, perhaps, for the apparent anti-Pentagon bias of southern districts holding the other explanatory factors constant); and the standard error of the regression suggests that the purging is quite efficient.[24]

Table A-1. Purging Regression for Constituency Opinions

Variable	Parameter Estimates
Intercept	2.166 (.325)
Population change	.00651 (.00215)
Pentagon outlays	.287 (.155)
Tax burden	-.464 (.157)
Black	-.00173 (.00287)
South	-.280 (.132)
West	-.476 (.117)
Adjusted R^2	.71
Standard error of regression	1.12
N of observations	108

Note: Dependent variable is the estimated district mean constituency opinion on the NES defense spending scale. Each district is weighted by the reciprocal of the standard error of the estimated mean constituency opinion. Standard errors of parameter estimates are in parentheses.

Imputing Congressional Preferences

The method used here to estimate representatives' preferred levels of defense appropriations is essentially that set out by Krehbiel and Rivers (1988). We observe representatives' votes on a sequence of three roll calls offering alternative levels of total defense appropriations for FY 1982. The first vote (*Congressional Quarterly* roll call #302, 18 November 1981) was on an amendment to the appropriations bill proposing to cut 2% from funds appropriated for weapons procurement and research and development;[25] the second vote (*Congressional Quarterly* roll call #303, 18 November 1981) was on the

Table A-2. Probit Analyses of Defense Appropriations Preferences

Variable	Excluding Competitiveness	Including Competitiveness
Intercept	-1.031 (2.114)	-.910 (2.180)
Constituency opinion	2.157[a] (.908)	2.095[a] (.947)
Constituency × competitiveness	.000 (—)	.00183[a] (.00808)
Tax burden	-.639 (.585)	-.673 (.605)
Pentagon outlays	1.289 (.578)	1.253 (.599)
Partisanship	.640 (.289)	.630 (.292)
Presidential influence	.802 .537	.764 (.561)
Threshold 1	.000 (—)	.000 (—)
Threshold 2	.183 (.082)	.183 (.082)
Threshold 3	1.148 (.168)	1.148 (.168)
Log likelihood	-91.0	-91.0
N of observations	96	96

Note: Standard errors of parameter estimates are in parentheses.
[a]Parameter estimates for purged versions of endogenous variables.

appropriations bill itself; and the third vote (*Congressional Quarterly*, roll call #315, 15 December 1981) was on the conference committee report reconciling the House and Senate appropriations.

With the first of these votes representatives faced a choice between the level of appropriations in the original bill, $197.44 billion, and a reduced level of appropriations amounting to approximately $195.78 billion; with the second vote the choice was between $197.44 billion and some unspecified (presumably uncertain) reversion level of appropriations; with the third vote the choice was between the conference report recommendation of $199.69 billion (a compromise between the House's $197.44 billion and the Senate's $208.68 billion) and the unspecified reversion level. Assuming sincere voting based on symmetric single-peaked utility functions, the corresponding indifference thresholds for the three votes were $196.61 billion, $98.72 billion plus Q/2, and $99.85 billion plus Q/2, respectively, where Q is the implicit reversion level of spending, given the failure of either the appropriations bill or the conference report.[26]

Given the assumptions of the model, representatives' observed patterns of votes on the three bills can be related to a series of explanatory variables (measuring each representative's political circumstances and district characteristics) by an ordered probit model. Table A-2 shows the probit parameter estimates for two versions of such a model, one excluding, and the other including, an interaction between constituency opinion and electoral competitiveness. The estimated interaction effect in the second specification is trivial in magnitude; thus, the pattern of parameter estimates for the other explanatory variables is similar across the two specifications.[27]

The estimated threshold values from the ordered probit model shown in Table A-2 can be used in conjunction with what we know about the content of the three bills to estimate both the relationship between dollar amounts and positions on the underlying continuum of support in Figure 3 and the implicit reversion level Q.[28] The estimated reversion level, $181.7 billion, corresponds closely to the figure of $183.4 billion obtained by simply adjusting the FY 1981 appropriation (including the $11.57 billion supplemental appropriation voted after Reagan took office) for inflation. The distribution of imputed spending preferences, shown in Figure 3, also seems reassuringly plausible.[29]

Notes

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1. The relevant literature is voluminous; a sample of important work might include Achen 1978, Converse and Pierce 1986, Erikson 1978, Fiorina 1974, Fowler and Shaiko 1987, Jackson 1974, Jackson and King 1989, Miller and Stokes 1963, Rivers n.d., and Rothenberg 1989. All of these studies are interesting and useful, but most fall short of providing concrete estimates of the effect of constituency opinion on legislators' behavior of the sort advocated here, often because they measured only indirectly or in very aggregated form constituency opinions or legislators' behavior or both. Statistical analyses of roll call votes have often employed very broad-guaged measures of district characteristics (such as mean demographic scores) as proxies for constituency opinions or interests. Conversely, analysts employing more direct measures of constituency opinion derived from survey data have often relied on roll call scales constructed for other purposes and have seldom provided descriptions of the specific votes that make up the scales or, more importantly, substantive interpretation of the magnitudes of the estimated relationships.

2. See, e.g., Mayhew 1974 on "vanishing marginals," Gelman and King 1990 on increasing "incumbency advantage," and Ferejohn and Calvert 1984 on declining "coattails" linking presidential and congressional candidates. Since 1980, about 80% of the senators and 95% of the representatives seeking reelection have been successful, with at least two-thirds of these piling up margins exceeding 20% of the general election vote (Ornstein, Mann, and Malbin 1990, 56–60).

3. From 1870 to 1950 net partisan seat shifts of 20%–30% over one or two election cycles were not uncommon in the House, and the average net seat shift in each election amounted to about 10% of the total membership. Since 1950 the partisan composition of the House has stabilized markedly. Only 3 of the last 20 congressional elections have produced net partisan seat shifts of 10% or more; and all three were reactions from, rather than accompaniments of, presidential landslides.

4. Even a political movement as potent as the "Reagan revolution" had only marginal effects in the House: the Republicans netted 34 new seats in 1980 (about 8% of the total House membership), and lost 25 of those 34 seats two years later.

5. The opinion trends plotted in Figure 2 are from the General Social Survey (GSS) and Roper (reported by Niemi, Mueller, and Smith 1989, 87) and from Gallup (reported in various issues of *The Gallup Poll*). In each case the indicated percentage is the margin believing that "we" (GSS/Roper) or "the government in Washington" (Gallup) are spending "too much (−) or "too little" (+) on "the military, armaments and defense" (GSS/Roper) or "national defense and military purposes" (Gallup). Typically, one-third to one-half of the public thought current spending levels were "about right" or didn't know whether spending should be increased or decreased.

6. Congressional Quarterly 1981, 311–12. The budget figures cited here include military construction and expected supplemental spending, which were not included in the main defense appropriations bill. Later in the year, in the face of increasing budgetary pressure, the administration's request was scaled back by $8 billion, with about a quarter of that amount reflecting reductions in actual FY 1982 outlays.

7. These amendments focused sometimes on total dollar amounts, sometimes on specific weapons programs, and sometimes on proposals to reform Pentagon procedures. Many were offered by staunch liberals (notably Patricia Schroeder, Democrat, Colorado); the few that were eventually adopted were offered by moderates from both parties.

8. The three roll calls analyzed here (*Congressional Quarterly* 1981 House roll calls #302, #303,

and #345) were chosen as the best available indicators of representatives' preferences over alternative levels of total defense spending. More detailed descriptions of each roll call and of the estimation procedure are provided in the Appendix.

9. Figure 3 also shows the estimated threshold values of defense appropriations preferences—the underlying preference levels at which representatives are predicted to switch from support to opposition or vice versa—for each of the specific roll call votes on which the analysis is based. It is interesting to note that the estimated threshold value for the key amendment attempting to contain the Reagan Pentagon buildup in the areas of weapons procurement and research and development (*Congressional Quarterly* roll call #302) is within about a billion dollars of the estimated mean (and median) legislator's preference; in the event, that amendment failed on the House floor by just five votes (197-202).

10. Both the estimated preferences shown in Figure 3 and the estimated effects of district characteristics shown in Table 1 are based on the probit analysis ("including competitiveness") reported in Table A-2 in the Appendix.

11. Several analysts (e.g., Achen 1978; Converse and Pierce 1986; Rivers n.d.) have attempted with some success to distinguish the impact of different constituents' opinions on representatives' behavior. Thus, the views of partisan supporters may be weighted more heavily than those of partisan opponents, or representatives may be most responsive to wealthier, more educated, and more politically active constituents. By contrast, my focus here on the mean constituency opinion in each district implies that representatives treat all constituents' views as equally relevant. This simplification is imposed in part for theoretical reasons (to approximate the ideal of responsiveness suggested by democratic theory) and in part because of data limitations (especially the relatively small number of individual respondents in each district).

12. The single exception is Charles Rangel (Democrat, New York), whose estimated constituency mean on the NES defense spending scale (based on eight responses) was −1.25; the other 107 estimated constituency means ranged from .14 to 2.25. Omitting Rangel from the analysis would leave all of the results essentially unchanged.

13. Although no full-scale analysis of the sources of constituency support for defense spending is offered here, the purging regression reported in Table A-1 in the Appendix combined with the estimated effect of constituency opinion in Table 1 suggests that these indirect effects of economic interests are on the order of $4 billion for a thousand-dollar per capita difference in Defense Department outlays and −$6 billion for a thousand-dollar per capita difference in federal tax payments, making the total effects in each case on the order of $10-12 billion.

14. Reagan received more votes than the winning congressional candidate in only 20 of the 108 districts in the NES sample, despite the fact that the total vote cast for presidential candidates is significantly greater than the vote cast for congressional candidates in most districts.

15. My inflation adjustment is based on a 7.07% increase in the price index for government purchases of goods and services in 1982.

16. President Reagan and his conservative allies in Congress significantly scaled back their defense spending proposals between the spring and autumn of 1981. Some of these concessions presumably reflected compromise with Pentagon opponents in Congress; but journalistic accounts emphasized the role of growing budgetary pressures even within the pro-Pentagon coalition.

17. The nominal increases in appropriations from FY 1981 to FY 1982 were 44% for procurement, 24% for research and development, and 16% for current operations (personnel, operations and maintenance, and pensions). The emphasis on new procurement and long-range investment in FY 1981 and FY 1982 may provide one explanation for the fact—evident from a comparison of Figures 1 and 2—that Pentagon appropriations continued to expand for several years after public opinion favoring such expansion had evaporated. Appropriations in the first year of the Reagan administration for the MX missile and B1 bomber programs, among others, produced significant momentum for additional spending on those programs in subsequent years.

18. For example, part of the real increase unaccounted for in Table 2 may reflect unmeasured presidential influence. I have estimated how representatives' positions changed as their vote totals relative to Reagan's vote in their districts changed; but it is impossible given the available data to estimate Reagan's absolute impact. The baseline I have adopted for the calculations in Table 2 supposes that Reagan was totally without influence in districts where he ran no better in 1980 (relative to the winning congressional candidate) than Ford had run in 1976. This somewhat arbitrary baseline probably underestimates Reagan's actual influence on the defense appropriations process in 1981.

19. In order to test the scope of congressional representation across a range of more specific defense issues, I replicated the analysis described earlier for 25 additional House roll call votes—all of the votes on defense issues in 1981 that appeared to have any significant budgetary implications. A detailed description of this supplementary analysis is omitted here but is available upon request. The most notable finding is that constituency opinion had significant effects on roll call votes across the whole spectrum of defense spending decisions, whether they involved specific weapons programs, military construction projects, or Pentagon purchasing procedures. On 22 of the 25 roll calls, however arcane the specific policy at issue, representatives' votes were

strongly and positively related to the positions of their constituents on the NES defense spending scale. This fact seems to confirm the appropriateness of thinking about "defense policy" (or at least "defense budget policy") as a unitary dimension; it also emphasizes the very considerable scope of congressional responsiveness to variations in relevant district opinion.

20. A notable exception to this generalization is the recent work by Jackson and King (1989) on tax policy, which is similar in its aims—and in many of its qualitative conclusions—to the work reported here.

21. Since the early 1970s the GSS has regularly measured public opinion favoring increases or decreases in government spending not only on defense but also on space exploration, foreign aid, education, environmental protection, national health, the problems of big cities, crime, drug addiction, welfare, and the condition of blacks. None of the other issue areas has seen a change in aggregate opinion approaching in magnitude the change shown for the defense spending issue in Figure 2. For these and other relevant data, see Niemi, Mueller, and Smith 1989.

22. An influential typology of representatives' goals and an analysis of the consequences of those goals for congressional behavior was provided by Fenno (1973).

23. The same districts were included in the 1978 NES, but defense spending was not among the issues constituents were asked about in 1978.

24. The standard error of this weighted regression due to measurement error alone would be 1.00; thus, the error introduced by purging amounts only to an additional 12%.

25. The vote was on Roukema's (Republican, New Jersey) substitute for Schroeder's (Democrat, Colorado) amendment proposing to reduce the appropriations for procurement, research and development, and testing and evaluation. Roukema's substitute proposed a 2% cut rather than the 5% cut proposed in Schroeder's amendment; it also exempted funds for procurement of spare parts, repair parts, and ammunition. The relevant actors seem to have assumed that Schroeder's amendment itself would fail (as it eventually did by voice vote), making the real alternative to the Roukema substitute the level of appropriations in the unamended bill. Both the announced position of the administration in opposition to Roukema's substitute and the observed pattern of support and opposition in the House lend support to this interpretation.

26. Obviously, n. 25 indicates that voting on the Roukema substitute was in fact strategic; but if my interpretation is correct, it was strategic voting of a straightforward (and readily incorporated) sort. There is no other indication of strategic voting in the roll call data. Given my interpretation in the text of the Roukema vote, four of the eight possible patterns of votes on the three separate roll calls are consistent with the assumption of single-peaked preferences over a single underlying dimension of preferred defense appropriations levels (and thus with the approach taken here of estimating unobserved ideal points by reference to observed threshold values). Each of the 96 congressmen in the NES sample who participated in all three of the relevant roll calls displayed one of these four consistent voting patterns.

27. The interaction between constituency opinion and electoral competitiveness, like constituency opinion itself, is treated here as endogenous. The corresponding regressor in the analysis reported in Table A-2, col. 2 is based on a weighted purging regression paralleling the weighted purging regression for constituency opinion shown in Table A-1. An unweighted version of the purging regression produced similar results.

28. We have a system of equations in which three pieces of information (the threshold values in Table A-2) can be used to solve for three unknowns (the intercept and slope of the equation relating probit scores to dollar amounts and the implicit reversion level Q). In Krehbiel and Rivers 1988 the reversion level is taken as given; here there is no obviously reasonable assumption about the final outcome, given the failure of either the appropriations bill or the conference report.

29. Spending preferences were imputed to each representative by multiplying the representatives' probit score (calculated using the probit parameter estimates in Table A-2, col. 2) by $6.145 billion and adding $189.555 billion. (These conversion values were computed from the estimated probit threshold values in Table A-2 using the system of equations referred to in n. 28.)

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POLARITY, THE OFFENSE-DEFENSE BALANCE, AND WAR

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Bipolar systems are inherently more stable than multipolar configurations of power, Kenneth Waltz argues. His empirical justification for this conclusion relies on the multipolar systems that preceded the two world wars and the bipolar Cold War. The weakness of Waltz's argument is the small number of cases and the failure to consider alternative explanations for different levels of war in the three periods. In another historical period of both multi- and bipolarity—Europe from 1495 to 1559—I have found that polarity cannot account for the constant level of instability across a change in polarity in the system. Instead, the offense-defense balance, which includes the technical military balance, the cumulativity of power resources, and strategic beliefs, explains instability in the period. Drawing on this alternative theory, I reassess the high level of stability associated with the Cold War and speculate on the level of stability we can expect in the post-Cold War period.

In *Theory of International Politics* (1979) Kenneth Waltz argues, that a single variable, polarity, can explain the level of instability, or war, in the world. He argues that this instability is associated with great powers' strategies toward each other and toward their allies. In limiting himself to the single case of post-World War II bipolarity, Waltz weakens the presumptive general validity of his theory. The evidence from sixteenth-century Europe shows that polarity does not explain the level of instability in an international system, though it does partially explain alliance strategy. Instead, the level of instability in multipolar Europe of 1495-1521 and bipolar Europe of 1521-59 can be explained by a comprehensive definition of the *offense-defense balance* in the period.

The state of the technical offense-defense military balance, the ease with which power resources cumulate, and the nature of strategic beliefs about the international system combine to form a more powerful theory of international politics for the period. But since sixteenth-century Europe also is only a single case and the level of instability does not vary significantly across these two periods of different polarity, it is not possible to assert with confidence that the offense-defense balance is either a necessary or a sufficient condition for the observed outcomes. In sum, while confidence in the validity of polarity as a predictor of important international phenomena is seriously challenged, solid support for the alternative explanations still awaits definitive demonstration.

I shall review the claims Waltz makes for the explanatory power of polarity, propose an alternative explanation, the offense-defense balance, comprising the technical military balance, the cumulativity of power resources, and strategic beliefs, and test the competing explanations against the historical record of multipolar Europe of 1495-1521 and bipolar Europe of 1521-59. I shall conclude

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with a reinterpretation of the long peace and the post-Cold War world based on the findings generated by this case study.

The Predictions of Polarity

International Stability

Waltz argues that a bipolar system is more stable than a multipolar one and defines *stability* as changes in the number of poles (1979, 135). Waltz, however, also acknowledges that stability is the avoidance of great power war, or war between the poles (p. 202). I use a yet-broader definition of instability, one developed by Jack Levy (1983), because it provides a more comprehensive set of instability indicators against which to test the competing theories than a change in the number of poles or the presence of interpolar war.

Levy identifies four ways to measure instability. These are the frequency, magnitude, duration, and severity of war. *Frequency* refers to how often war breaks out over a given period; *magnitude* denotes how many poles are involved in a given war; *duration* refers to the length of wars; and *severity* is measured in terms of battle deaths per war-year in proportion to the populations of the involved states.

Alliance Strategies

For Waltz, the relative importance of allies is an intervening variable between polarity and instability. He argues that alliance strategies in a bipolar system have a benign effect on systematic stability, while under multipolarity such strategies lead to wars involving the poles.

In bipolar systems, allies are unimportant, hence there is no destabilizing competition for allies. Moreover, allies' interests are not considered when developing grand strategy; thus, the poles can concentrate on countering the other's power. Under multipolarity, there are too many poles to permit any of them to draw clear and fixed lines between allies and enemies and to be certain of what is happening in the system. But there are too few poles for a state to remain indifferent to the defection of an ally. Hence, there is a tendency to pass the buck (rather than balance against emerging hegemonic challengers) and to make one's strategy hostage to other states (Waltz 1979, 163-71).

An Alternative Theory: The Offense-Defense Balance

The offense-defense balance consists of three elements. The first is the technical offense-defense military balance concerning the relative military advantages enjoyed by the offense or defense on the battlefield, that is, castles versus cannons, machine guns versus trenches, and so on (Quester 1977; Levy 1984, 223-25). The second element is the cumulativity of power resources, or the relative availability of the resources that underlay military capability and the relative ease of their extraction by occupying states. The third element is the set of strategic beliefs held by the leaders of the great powers, their relative concern for their reputation, or credibility.[1]

The Technical Offense-Defense Military Balance

Offensive and defensive advantage should be separated into tactical and strategic categories. Tactical offensive advantage is the ability to seize a piece of an enemy's territory at less cost to oneself than it requires for the defender to protect it or retrieve it. This approximates Mearsheimer's (1983) notion of "limited aims." A strategic offensive advantage is the ability to seize and/or occupy as much of an enemy's territory as is necessary to destroy its military potential at less cost to oneself than is required for the defender to protect its territory or retake it. The deduced consequences of the military offense–defense balance for the different elements of instability are shown in Table 1.

In the case of both tactical and strategic offensive advantage, one should expect frequent wars, since a state can expect both short- and long-term gains from initiating the war. But the wars should be short, as the attacker will be able to overrun any opposition quickly. Battle casualties should be high, given the disproportionate losses generally suffered by an army on the attack. There should be a greater incidence of interpolar wars, given the enormous promised gains from victory.

Where there is an advantage for the strategic offensive, but a tactical defensive advantage, wars will still be frequent, given the enormous gains promised by the ease of conquest once the initial resistance is overcome, but not as frequent as in the former, doubly offensive case. Wars will be long, due to that initial resistance; and casualties should therefore be more severe than in the previous case as well. The prospect of high gains will inhere again to greater interpolar warfare.

In the case of strategic defensive advantage and tactical offensive advantage, wars will be frequent, due to the ability to seize and hold territory, but not as frequent as the first two cases, since the incentives for warfare are less. Wars should be short, because as soon as the limited aims are achieved, there is no further advantage to be gained from continuing warfare. Casualties will be low, due to the relative ease of attaining limited territorial gains. Interpolar wars should be less frequent, given the lack of prospective strategic gains from such wars.

Where there is both strategic and tactical defensive advantage, wars will be infrequent due to the double disincentive. What wars do occur will be short, due to the relative futility of trying to press home the attack. Casualties will be low, due to the absence of strategic warfare, but will be higher than the previous case, due to the losses suffered while trying to achieve limited aims against the advantaged defender. There will be still fewer cases of interpolar war than in the previous case, due to the same double disincentive.

Table 1. Effects of the Offense–Defense Military Balance on Instability

Strategic Advantage to the	Tactical Advantage to the: Offense	Tactical Advantage to the: Defense
Offensive		
Frequency	high	high
Duration	short	long
Severity	high	high
Magnitude	high	high
Defensive		
Frequency	high	low
Duration	short	short
Severity	low	low
Magnitude	low	low

The Cumulativity of Power Resources

If military technology were the only determinant of the balance between offense and defense in the international system, Table 1 would be sufficient to predict the level of instability. But the potential value of the conquest itself must be included in a more comprehensive conceptualization of the balance. This *cumulativity of power resources* includes both the *availability* and the *extractability* of power resources.

Available power resources are the material elements that constitute military and economic power in a given historical period. The resources of power and their geographical locales change over time. In the nineteenth century, for example, if Germany were to contemplate attacking another state, it would consider the grain, coal, iron ore, and industrial endowments of that state; it would not consider petroleum, bauxite, and uranium deposits to be of much relevance. The greater the relevance of another state's resources to increasing one's own power, the greater the incentive to go to war. For instance, competing great powers lost interest in Bohemian copper and tin mines after iron replaced bronze in the manufacture of cannon and cannonballs in the late sixteenth century (McNeill 1982, 87–89). In terms of Table 1, all elements of instability should be exacerbated to the degree that resources of power are available in other states in the system.

The extractive power of the potential conquerer includes the relative ease of occupation or administration and the relative ability to transfer whatever power resources are available to the conquering state. One should expect that the lower the costs of occupation and the more readily transferable the resources of power, the greater the instability in the system.

Strategic Beliefs

The ideas held by the ruling elites of great powers about how the international system operates comprise three elements that can vary along the offense–defense continuum. Offensive strategic beliefs assume rulers to be very concerned that if they allow another pole to gain a military victory anywhere in the international arena, these opposing rulers will learn three lessons that will redound to the disadvantage of the defeated state: (1) the rulers of the victorious state will believe that it can successfully challenge the positions of the recently defeated state even in areas of the globe that are strategically vital to the defending state; (2) the defeated state's allies will begin to question the advisability of relying on the former's security guarantees, given its recent record, and hence will tend to bandwagon with, rather than balance against, any future efforts at expansion by the recent aggressor; (3) states that are located in the neighborhood of the recent victory will fall like dominoes into the lap of the aggressor, rather than redoubling their efforts to balance against this ascendant threat. Defensive beliefs, on the other hand, assume rulers to be unconcerned about their credibility, confident that allies will balance, not bandwagon, and that dominoes will not fall after their adversary's victories.

Defining a Pole

Waltz offers two different sets of criteria for identifying a state as a pole. Under conditions of bipolarity, no third power should be able to challenge the top two (Waltz 1979, 98). This implies that Waltz's unit of value is limited to economic and military capabilities. But he also argues that to qualify as a pole, a state must be measured in terms of size of population and territory, resource endowment, economic capability, military power, political stability, and competence (p. 131).

Military power, however, is the resultant of the other six elements, not an equivalent part of a bundle of power. Indeed, much of Waltz's theory rests on precisely the assumption that military power is the only capability that merits measurement in determining a state's candidacy for polar status.[2] Moreover, it is hard to acquit Waltz of the charge of committing a "reductionist" error in terming political stability and competence as systemic variables.

I adopt Waltz's core argument that to determine the polarity of an international system, one must measure the distribution of military capabilities among the actors in that system. However, while accepting this sparest of measures, in order to make my definition more comprehensive, I also use population figures and government

Table 2. The Distribution of Power in Multipolar Europe, 1495-1521

	Population		Soldiers		Ships		Revenue	
State	Millions	%	Thousands	%	Number	%	Millions of Ducats	%
Austria	12	21	16	9	0	0	1.0	6
England	4	7	27	16	15	4	1.2	7
France	15	26	35	20	20	6	5.4	33
Spain	6	10	16	9	90	26	1.5	9
Ottoman Empire	19	33	60	34	80	24	5.4[a]	33
Venice	1.6	3	20	12	135	40	1.7	10
Total	57.6	100	174	100	340	100	16.2	100
Average	9.6	17	29	17	57	17	2.7	17

[a]Estimate.

revenues to determine a state's polar status.[3]

Multipolar Europe, 1495-1521

The European system was multipolar from 1495 to 1521. Prior to Charles V's consolidation of the Habsburg Empire, there were six states of relatively equal military power: England, France, Habsburg Austria/Germany, Spain, Venice, and the Ottoman Empire. Table 2 shows the distribution of capabilities for the period of multipolarity. Each of the six states in the multipolar period ranked in the top three in at least one category of power. Austria, while it lagged in military power and treasury receipts, had the third largest population.[4] England had a relatively small population, navy, and revenue base, but it fielded the third largest army in the period (Oman 1937, 291-305; Wernham 1971, 62, 87, 93). France, while first in revenue, and second in population and size of army, was near the bottom in terms of the major strategic weapon of the day—the ocean-going warship (Ady 1957, 359-63; Ehrenberg 1928, 74-75, 204-6, 289; Knecht 1984, 20-49; Laffan 1957, 200-207; Oman 1937, 26-174). Spain, with few people, revenues, and soldiers, still deployed the second-largest navy in the period (Batista i Roca 1957, 333-34; Brandi 1939, 88-89; Cowie 1977, 116-17; Hale 1957, 282-87; Oman 1937, 51-57, 124-48).

The Ottoman Empire was strong in all categories of power, though its revenues could only be indirectly measured as a product of its output, military power (Braudel 1972, 2:668, 696, 720-21; Fisher 1957, 33-34; Kortepeter 1972, 4-11; Lane 1973, 242, 369). Finally, Venice, though last in population and nearly last in revenues and number of men under arms, still deployed the largest navy in the period by far (Lane 1934, 46-48, 60, 101-10, 138; Lane 1973, 359-65; Mallett and Hale 1984, 131, 474).

Bipolar Europe, 1521-59

The European system was bipolar from 1521 to 1559, the two poles being the Habsburg and Ottoman Empires. Table 3 displays the distribution of capabilities in the period. By 1521, Charles V was the ruler of Spain, the Netherlands, Habsburg Austria and Germany, Sicily, and Naples; and all the rest of Italy paid some form of tribute to him, with the exception of Venice. While the average size of his army totaled 62 thousand men, several times in the period he assembled armies of over 100 thousand.[5] The emperor also regu-

Table 3. The Distribution of Power in Bipolar Europe, 1521–59

	Population		Soldiers		Ships		Revenue	
State	Millions	%	Thousands	%	Number	%	Millions of Ducats	%
Habsburg Empire	30.4	40	62	29	200+	32	8.5	32
Ottoman Empire	21.0	28	74	35	200+	32+	9.5	36
France	17.0	22	32	15	20	3	5.0	19
England	6.0	8	31	15	100	16	2.1	8
Venice	1.6	2	12	6	100	16	1.5	6
Total	76.0	100	211	100	620+	100	26.6	100
Average	76.0	20	42	20	124	20	5.3	20

larly raised fleets of more than two hundred ships.[6] Perhaps the truest measure of Charles V's power in the period was his ability to raise money on foreign capital markets. From 1521 to 1556, he raised a total of 45 million ducats from German and Genoese bankers.[7]

The Ottomans, as before, deployed the largest armies in Europe. They often exceeded 100 thousand men, the largest being 150 thousand during one of Suleiman's campaigns up the Danube. In addition, the sultan was able to assemble fleets of more than two hundred vessels in the Mediterranean (Merriman 1944, 64–67, 79–112, 155, 213, 238–49; Oman 1937, 657, 690–700; Parry 1958, 514–25).

France slipped to nonpolar status in this period. Its average army was barely half the size of imperial forces. Indeed, it exceeded the imperial average only once, in 1543–44, when Francis I mustered almost 75 thousand men to thwart the combined invasion of Charles V and Henry VIII. Unlike the Habsburg and Ottoman poles, and even nonpolar England and Venice, France lacked any significant naval power (Braudel 1972, 2:904, 921–29, 941; Hale 1958, 490–504; Oman 1937, 37, 46, 65, 79, 175–97, 232, 254–77, 292, 341–46, 700; Wernham 1971, 156–73). Its revenues also declined from the previous period.[8]

England under Henry VIII greatly expanded its military capabilities in the period, fielding armies equal to France's in size. This was accomplished by doubling the Crown's income. Henry VIII also began the first sustained English naval buildup. Though dwarfed by the later efforts of Elizabeth I in the second half of the century, Henry was able to deploy 100 ships by the 1540s.[9]

The Venetian Empire, as before, remained a naval power, its armies being negligible compared to England and France, let alone the Habsburgs and Ottomans. Even its hundred-galley standing reserve was outstripped in the Mediterranean by the naval forces of both the Habsburgs and Ottomans. Though its revenues increased by 50% in the period, they were not nearly sufficient to maintain polar status for Venice (Lane 1934, 107, 141–51; Lane 1973, 318–23; Mallett and Hale 1984, 474–80).

The shift from a multipolar Europe of six powers to a bipolar system dominated by the Habsburg and Ottoman Empires is manifest in several ways: one can (1) compare the total power in the system held by the top two states under the two different types of polarity; (2) consider the gap between the power held by these two states and the power of the next most powerful state in the system; or (3) compare the amount of power held by the Habsburg and Ottoman Empires to that held by the United States and Soviet Union in the postwar period.

The Top Two States. The shift from multi- to bipolarity is supported by the figures for population and army size and dramatically demonstrated by shares of strategic power. The evidence for revenues, however, is not consistent with such a shift; although, as noted above, the data on Ottoman revenue is only an approximation. In the multipolar period, the two most populous states, France and the Ottoman Empire, accounted for 59% of the system's total population. In the bipolar period, the Ottoman and Habsburg Empires had 68% of the total population. In the earlier period, France and the Ottoman Empire deployed 54% of the system's soldiers, while in the subsequent period, the two dominant empires fielded 64% of the system's armies.

The biggest difference between the two periods is in the area of strategic weaponry. In terms of naval power, although there was no change in the total amount controlled by the top two states, they were not the same two states. In the period of multipolarity, *Venice* and *Spain*, not the Ottoman Empire or France, combined to deploy 66% of the system's naval power. In contemporary perspective, this is as if the United States and Soviet Union were dominant in population, GNP, and armies, but France and China deployed two-thirds of the system's nuclear warheads. In the subsequent period, the Habsburgs and Ottomans possessed 64% of the system's warships. Only in terms of revenues were both systems bipolar.

The Third Top State. Another way of measuring the system's bipolarity is to assess the gap in power between the weaker of the top two states and the third most powerful. In a bipolar configuration of power, there should be a very large difference; while under multipolarity, there should be no gap. The data on military power strongly confirm a shift to bipolarity in 1521. Population figures are mildly inconsistent, while the change in the revenue gap is very inconsistent with such a shift.

France's average army of the multipolar period, 35 thousand men, was only 30% bigger than England's army of 27 thousand. In the bipolar period, the Habsburg army of 62 thousand was almost twice as large as its French counterpart. In the multipolar period, Spain's navy of 90 ships was a mere 12.5% larger than the Ottoman Empire's fleet. In contradistinction, the naval power of the Habsburg and Ottoman Empires was fully twice the size of English and Venetian naval forces in the bipolar period.

In the period of multipolarity, Austria's population of 12 million was only 20% less than France's population of 15 million. Subsequently, France's population of 17 million was 19% less than the Ottoman Empire's. In the multipolar era, French revenues were a very large 218% greater than Venetian income of 1.7 million ducats per year. In the subsequent period, Habsburg revenues were only 70% greater than French income.

The Post-World War II System. Finally, one can compare the distribution of capabilities in Europe from 1521–1559, to the degree of bipolarity in the post–World War II world, Waltz's paradigmatic case. In fact, the sixteenth-century case is *more* bipolar than the postwar system.[10] In terms of population, the Habsburg and Ottoman Empires are ranked first and second, respectively. The *combined* population of the Soviet Union and the United States is only one-half of China's 1.1 billion and only two-thirds of India's 810 million. Habsburg economic power, as measured by government revenues, was 70% more than French income. The Soviet GNP of $2.2 trillion is almost $800 billion (more than 26%) *less* than Japan's GNP of almost $3 trillion. The average Habsburg army was almost *twice* the size of the average French army. The U.S.

army of 767 thousand is almost three times *smaller* than the Chinese army, less than 70% the size of the Indian and Vietnamese armies, 80% the size of Iraq's army, and only 31% larger than the Turkish army.

The only area in which bipolarity is deeper today than it was in sixteenth-century Europe is in strategic weaponry. While the average Habsburg or Ottoman fleet was more than twice as large as the Venetian or English navies and more than 10 times more powerful than French naval efforts, Soviet strategic nuclear forces today, numbering 12,400 strategic warheads, are 15 times larger than the British forces of 817 warheads and 20 times the size of the French nuclear force. And even here, given the reductions foreseen by START I and II, and the planned deployments of France and Britain over the next decade, the gap between the Soviet Union and the third-ranking nuclear power will get much smaller.

In sum, the European system was multipolar from 1495 to 1521 and bipolar from 1521 to 1559. Moreover, that bipolarity was even deeper in terms of population, economic power, and army size than post-World War II bipolarity. The only exception is in strategic weaponry, though there was still a sizable gap between the Habsburgs or Ottomans and their nearest competitors, and the gap between the Soviet Union and its nuclear counterparts is closing fast.

Testing the Alternative Theories

Multipolar Europe, 1495-1521

The offense-defense balance predicts a significant amount of instability in this period due to tactical offensive military advantages, available power resources, and offensive strategic beliefs. This instability should be mitigated, however, by the facts that the strategic defensive has the advantage and that power resources are not easily extracted. Waltz's theory of polarity would simply predict greater instability in this period than in the subsequent, bipolar period.

The Technical Military Balance. Levy (1984) concludes that there was a historical consensus that the European system was offense-dominant from 1495 to 1525. He does not, however, distinguish between the strategic and tactical components of this concept. In fact, the system should be characterized as tactically offensive, but strategically defensive. The offense was tactically advantaged for several reasons (Hale 1957, 1958; Mallett and Hale 1984; Nef 1950; Oman 1924, vol. 2; Oman 1937).

Military technology favored the tactical offensive. Artillery demonstrated the weakness of fortifications against metal cannonballs. Bastioned fortifications, affording defensive advantage, did not become commonplace until the early seventeenth century. The defensive advantage of the pike square gave way to the offensive combination of field artillery, arquebuses, and heavy cavalry assaults. Artillery was also useful in driving an opponent from his fixed position into cavalry assaults.

There were many reasons why this period was characterized by strategic defensive advantage (Hale 1958; Laffan 1957; Lockyer 1974; McNeill 1982; Merriman 1944; Parry 1958; Petrie 1949). The first concerns the character of mercenary armies. The latter often refused to fight after being refused pay increases on the battlefield. Beyond their unreliability, mercenary armies also hindered strategic gains by their method of financing. The promise of plunder and loot was the cheapest way to pay for mercenaries' services. It was hard to press any attack when one's soldiers disintegrated in search of booty, rather than taking advantage of a strategic breakthrough. Such plundering of the countryside so alienated the victimized populations that the strategic strength of armies was dissipated by the need to leave garrisons for policing duties along all axes of advance.

In addition, the need to besiege fortified towns drained state treasuries quickly, since each episode was a lengthy affair. So, even if cannonfire ultimately could reduce the walls of any town, it was an enormous burden on the finances of the warring state and significantly reduced the length of time any army could sustain a campaign.

The other time factor that contributed to the advantage of strategic defense was the change of seasons. Winter was perhaps the greatest obstacle to any definitive defeat of another pole. The Ottoman wars in this period are graphic examples of this insurmountable disadvantage. A major offensive up the Danube was hard enough, with rivers to be forded and swamps to be traversed. Vast stores of food had to be brought along to guard against starvation caused by the onset of winter and the scorched earth tactics of the defenders. Each campaign in this period had to be broken off due to these supply problems, no matter how close to strategic victory the sultan's forces might be. This is why Ottoman campaigns generally lasted only three to four months a year, hardly sufficient time to score a strategic victory. This world of advantages to the tactical offensive, but also to the strategic defensive, predicts frequent, though short, wars with low levels of casualties and of great power involvement.

The Cumulativity of Power Resources. In terms of the ease with which power resources cumulated, this period was mixed. Power resources were available, but their extraction was a difficult and costly matter. It was generally believed that money was interchangeable with military power (Mattingly 1955).[11] And money could be acquired by conquest simply by becoming the new depository of a defeated population's tax payments. An attacking state could also receive money directly from its victim in exchange for allowing the latter to survive. Charles VIII, for example, was paid 250 thousand ducats by the Firenzans to spare their city; and Ferdinand V was paid 500 thousand ducats by the residents of Oran for the same favor. If one recalls that Spain's average revenue was 1.5 million ducats per year, one can understand the enormous attraction of prospective gains from war. The great powers were also attracted by the prospect of gaining control over important grain producers, a factor that fueled the lengthy French-Spanish struggle over Abruzzi and Apulia.

While there were significant power resources available in the system to create incentives for war, the occupation and administration of these territories were difficult.[12] Excessively harsh or exploitative rule gave rise to unrest, which invited other states to try to replace the present occupier. Though the French are widely regarded as having been the least adept at holding on to their gains, Spain too suffered revolts.

Brutal occupation policies had a two-edged effect on the offense-defense balance in the system. It made strategic gains far less likely, but it also made wars aimed at supplanting present rulers far more fruitful gambits.

In overall terms, the cumulativity of power resources in this period can provide only indeterminate predictions about the level of instability in the system. On the one hand, material power resources themselves were abundant, available, and fungible into wealth and military capability. On the other hand, these gains proved to be ephemeral due to inept and brutal occupation policies by the conquerors, which made extraction very problematic.

Strategic Beliefs. The strategic beliefs held by statesmen in this period also appear to push the system toward offense-dominance. There is evidence (admittedly quite thin) that there was fear that dominoes would fall and fear that allies would bandwagon, either of which would require a military response.

Max Bergher, lord of Zevenbergen, a Dutch nobleman, wrote to Charles V in 1519 to argue for the acquisition of Württemberg. His reasoning was that if Württemberg were not taken, it would "join with the Swiss Confederation, which could not help but draw in Swabia and the Rhineland as far as Cologne." Given this, it was equally important to maintain control of Hagenau, for otherwise the Decapolis (a group of Alsatian principalities) would "join with the Swiss and what would then become of Strasbourg may well be conceived" (Brandi 1939, 121). This is a pure image of falling dominoes, starting with Württemberg in present Southwest Germany through contiguous territories up the Rhine River valley to Strasbourg and beyond.

Images of bandwagoning allies are evident in a 1496 letter from Maximilian to Ferdinand in which Maximilian urges the Spanish king to make war against France immediately, since the German princes would follow him if he prospered but would join with Charles VIII of France if he hesitated. As it was put during the period, a prince's "reputation" was important above all else (Koenigsberger 1975, 157).

According to the technical military balance alone, the multipolar European system from 1495 to 1521 should be characterized by frequent, short wars, with relatively low casualties and relatively few interpolar conflicts. There should be even greater instability than this, however, due to the offensive character of the strategic beliefs held by statesmen in the period. The effects of the nature of power resource cumulation are indeterminate. Polarity simply predicts more systemic instability under these multipolar conditions than in the bipolar conditions prevailing in the subsequent period.

Bipolar Europe, 1521–59

While Waltz would predict much more stability in this bipolar period, the offense–defense balance would not predict any significant change from the multipolar period. This period is one of strategic defensive dominance and growing tactical defensive advantage but also one where power resources are available and extracted with increasing ease. In addition, offensive strategic beliefs are the norm.

The Technical Military Balance. The offense–defense balance shifted marginally toward greater defensive advantage in this period as tactical gains became somewhat more difficult to achieve and strategic positions remained impregnable.

Plunder remained a major obstacle to strategic gains, with soldiers looting, rather than fighting.[13] Better fortifications also quickly dissipated the strategic strength of armies. Moreover, it was necessary to capture each one of these fortified towns along an axis of advance; otherwise, one's supply lines would be vulnerable. This required leaving more troops to garrison these newly won positions, further depleting the army's forces. As in the previous period, campaigns were limited by distance and weather. Neither Charles nor Suleiman the Magnificent were able to mount significant operations even one thousand miles from home.

The tactical defensive was strengthened somewhat in the period by using field fortifications and entrenched positions, the object being to induce one's enemy to attack unwisely against artillery and arquebus fire. As Oman put it, "Waiting became a much-used strategy" (1937, 38). In this sense, military technology favored he who had both the money and time to prepare defenses and wait out the attacking forces.

The Cumulativity of Power Resources. While military technology contributed to a marginal shift toward increased defensive advantage in this period of bipolarity, it simultaneously became somewhat easier to accumulate power resources. Power resources remained available, and their extraction became a bit less difficult.

How leaders in this period thought of the fruits of conquest is revealed in a letter from Charles V to his brother Ferdinand in Austria. In discussing his plans for Italy he wrote that he would try to

> reconcile the Venetians to myself by offering to ratify the last treaty that was made with them, as if they had never infringed it, provided they agree to pay me at least 100,000 ducats a year; [I] will place Duke Sforza in possession of Milan on condition he pays me 600,000 ducats and renounces in my favor all right to the Duchy of Bar . . . and that he provides for the distribution of the salt of Milan according to my directions. (Bradford 1850, 142)

The Ottoman campaigns up the Danube gave it access to the fertile plains of the river, increasing its revenues and grain supplies. Money and other power resources are both desired and available in this period.

There is also evidence that the other element of cumulativity, the relative ease and cost of extraction and occupation, began to favor the offense in the period. Apparently having learned the lessons of the previous period in Italy, Charles became quite adept at appointing natives to the public offices in his dominions. Visitors to Sicily were impressed that the local subjects expressed voluntary allegiance to the Habsburg Empire (Koenigsberger 1969).

Strategic Beliefs. Fear of bandwagoning allies and falling dominoes continued to be evident in the perceptions of statesmen under bipolarity. As Charles's ambassador to France, de Praet, reported to him in 1526: "As to the Italians who are here, they are talking of becoming as loyal to Your Majesty as they have hitherto been to the King of France, in hopes that each of them will recover his lands and houses" (Bradford 1850, 199). This bandwagoning mentality among the Italians occurred in the wake of the imperial capture of Francis I at Pavia. Charles's imperial councillor Gattinara argued in 1527 that once Charles had established his hegemony over all of Italy, all of his other dominions "would flock to do him service" (Brandi 1939, 257). While victories were perceived to lead to cumulating gains, it was feared that defeats would set off falling dominoes.

Don Diego Mendoza, Charles's ambassador to Venice, wrote to him in 1543 that he must never give up Milan, "[for] if it falls into the hands of France your friends will desert you on the [Italian] Peninsula." In his final testament before abdicating, Charles warned his son Phillip "never to yield an inch to France as they will take an ell [yard]" (Brandi 1939, 479, 584). Charles obviously believed in the need to maintain credibility in the eyes of the French.

According to the technical military balance alone, the period from 1521 to 1559 should see slightly fewer wars than the preceding period, due to the gradual advantages being gained by the tactical defense. However, power resources remained available and their ease of extraction increased. Strategic beliefs additionally contributed to offensive advantage. In overall terms, the offense-defense balance predicts very little, if any, change in the level of instability in the system. Polarity, on the contrary, predicts a marked reduction in war between the poles.

Table 4. Levels of Instability

Polarity Type	Factors of War Frequency (%)	Magnitude (% Interpolar Conflicts)	Average Duration (Months)	Average Severity (Casualties per War-Year)
Multipolar, 1495–1521	63	67	27	11,000 (.02% of population)
Bipolar, 1521–59	53	60	28	10,000 (.015% of population)

Sources: Braudel 1972, vol. 2; Cipolla 1976; Fisher 1957; Kellenbenz 1976; Mattingly 1955; Oman 1924, vol. 2; Oman 1937; Sorokin 1962, vol. 3.

The Empirical Evidence and the Competing Theories

Systemic Instability

The international situation in sixteenth-century Europe became only marginally more stable after the shift from multipolarity to bipolarity. This record completely contradicts the predictions of polarity. This lack of significant change is consistent with the overall state of the offense–defense balance in the system, which changed only marginally and in cross-cutting fashion. The evidence supports the conditions of the technical military balance and strategic beliefs as predictors of stability, but is inconsistent with the greater ease of accumulating power resources in the system. Of course, since this is only one case, I make no claims as to the general validity of the results for the alternative explanations; but polarity's validity as a predictor of systemic instability is clearly put in doubt.

The levels of instability in the two different periods of polarity are presented in Table 4. In the 26 years of multipolarity from 1495 to 1520 there is a maximum of 156 country-years of war (26 years multiplied by six poles). For example, the five years of war between Maximilian and Venice from 1508 to 1513 is actually 10 country-years of war. There were 98 country-years of war in this period, or almost 63% of the maximum possible. There were only two years of international peace in this period, in 1506 and 1520.

In the 39 bipolar years from 1521 to 1559 there is a maximum of 78 country-years of war between the two poles. However, there are also an additional 78 country-years of war that both poles can have with other nonpolar states in the system. Of these 156 possible country-years of war, there were 83 in this period, or somewhat more than one-half the possible frequency. There were only three years of international peace in the period. Hence, wars were only slightly less (16%) frequent under bipolarity than multipolarity.

As to magnitude, 66 of the 98 country-years of war in the multipolar period, or over two-thirds of the total, involved interpolar war, that is, war between two or more poles on one of their territories. The rest involved either wars fought by poles against each other but in third areas, like Milan, or against nonpoles, like Ferdinand's expeditions in Northern Africa. Fifty of the 83 country-years of war in the bipolar period, or more than 60%, involved interpolar war. Again, these figures evince only negligibly less (10%) instability across the two types of polarity.

In the multipolar period there were 26 discrete wars, lasting a total of 58 years. So, for this period, the average war lasted a little less than 27 months. In the bipolar period there were 25 separate wars lasting a total of 59 years, yielding an average duration of a little longer than 28 months. The number of casualties per war year and casualties as a ratio of total system population also did not vary significantly (9%) across the two periods.

The measures of instability used in this study did not vary significantly across the change in polarity in the system. The frequency and length of wars, their severity, and their magnitude all appear to be completely unrelated to the distribution of capabilities in the system. This constant level of instability is consistent with the overall unchanged value of the offense-defense balance in the period. While the technical military balance became somewhat more defensive across the two periods, power resources became more cumulative, and strategic beliefs remained offensive. In terms of the specific elements of the offense-defense balance, the evidence of relatively constant, though marginally diminished, instability is consistent with the change in the technical military balance, inconsistent with the change in the cumulativity of power resources, and indeterminate with respect to strategic beliefs.

It should be stressed once again, however, that the evidence from this single case is most powerful in calling into question the causal power of polarity as a predictor of instability, less powerful in validating the offense-defense balance as the alternative explanation, and much less powerful in providing support for individual elements of that balance.[14]

Alliance Strategies

Multipolar Europe, 1495–1521. Waltz argues that under multipolarity, due to uncertainty and relative equality in military capabilities, the great powers pass the buck, allowing a challenger for hegemony in the system to succeed. There also will be frequent great power wars due to the rigid strategies necessarily adopted by poles solicitous of their allies' concerns.

Waltz's predictions are contradicted by the empirical record of multipolar Europe. The most persistent threat to establish hegemony over the system arose from France and its adventures in Italy. These gambits were repeatedly blocked by various coalitions; and there was great variety, not rigidity, in the composition of alliances.

When Charles VIII invaded Italy in 1494, he was met by the League of Venice, comprising Spain, the Papacy, Austria, and Venice. By the end of 1495, Charles was forced to retreat across the Alps. After Venice defeated Austria in Venezia-Giulia and Friulia and appeared to be making a bid for the domination of a substantial piece of the Italian peninsula, the League of Cambrai was formed in 1508 to counter this effort. It consisted of the Papacy, Austria, France, and Spain, indicative of just how flexible alignments were in the period.

Bipolar Europe, 1521–59. Waltz's predictions for alliance strategy under bipolarity prove correct in this period. Both the Habsburgs and the Ottomans concentrated their military energies on weakening the other, and both treated their allies as desirable but not very important. Some of the more illustrative examples for Charles V are found in his treatment of England. He used Henry VIII's forces in Northern France as a diversion in 1522–23, so that he could clear France out of Northern Italy (Oman 1937, 323). He opposed Henry VIII's divorce of Catherine of Aragon, an attitude that drove England into alliance with France in 1527. Charles also made a separate peace with France in 1544 while Henry's forces were still besieging Boulogne, as Charles had already received his desired French concessions on Artois and Flanders.

Suleiman the Magnificent treated the French much as Charles treated England and Venice. He refused to send forces to help France when requested, while getting France to mount campaigns to divert Charles V during Ottoman marches up the Danube (Fisher 1957, 75; Merriman 1944, 132).

That the Ottomans were given the undivided attention of the Habsburgs is evidenced by the behavior of Charles's fractious German principalities. They were ever eager to accept French and English military support in their periodic revolts against Charles's orthodox Catholicism. But when any threat appeared from the East, they refused offers of such aid so as to unite against the common acknowledged threat. Even Martin Luther called on his cobelievers to support the Habsburgs against the Ottomans.[15]

Contrary to Waltz's concerns about the baleful effects of alliance strategies under multipolar conditions, the great powers in sixteenth-century Europe did not pass the buck when confronted by challenges for hegemony in the system. Instead, they continually balanced against the state that appeared the most threatening at any given moment. The offense-defense balance may help explain this balancing behavior.[16]

Christensen and Snyder (1990) argue that passing the buck is likely to occur when the system is defense-dominant, since it is clear that no state can feasibly execute a hegemonic strategy. When the system is offense-dominant, however, an equally pernicious strategy prevails—the creation of "chain gangs." Evocative of the alliance relationships prior to World War I, chain gangs lead to rigid strategies designed to ensure that no ally defects from a coalition. Statesmen are anxious about such defections because in an offense-dominant world, no state can afford to lose any potential ally.

The alliance strategies that dominate in multipolar Europe from 1495 to 1521 cannot be called either passing the buck or forming chain gangs. Instead, they appear to be optimal balancing strategies and exhibit none of the negative qualities associated with either of the other two. The nasty dynamics analyzed by Christensen and Snyder appear to refer to the *strategic* technical military balance. When the strategic offensive has the advantage, states form chain gangs; when the strategic defensive has the advantage, they pass the buck.

However, when there is strategic defensive advantage, but also *tactical offensive* advantage, states both avoid the complacency of passing the buck and the overreaction of forming chain gangs. Instead, they adopt sensible and effective balancing strategies.

Great powers operating under such conditions can afford to behave moderately, for their own homelands are not immediately at risk. However, they are also compelled to remain alert, since limited territorial gains that might cumulate are possible. Precisely such a technical military balance characterizes multipolar Europe from 1495–1521, and the alliance strategies pursued by the great powers are consistent with such a balance.

The behavior of the Habsburg and Ottoman empires from 1521 to 1559 validate Waltz's propositions about alliance strategies under bipolarity. Each pole regularly ignores the interests of its allies and concentrates its energies against the other. However, there is no evidence to support Waltz's posited connection between these alliance dynamics and reduced instability. Instead, levels of instability remained virtually constant across the two periods of polarity.

Neither is economic interdependence

(which Waltz argues mediates between polarity and war), a factor here. High levels of economic interdependence, which Waltz associates with multipolarity and greater instability, as well as low levels, are a function of the *size* of the units, not the distribution of power in the system. Bigger states, like the Habsburg and Ottoman empires, are more independent than the smaller states of Venice and England. In the multipolar period, interdependence correlated perfectly with size as well.[17]

Reinterpreting the Cold War and Its Aftermath

In light of polarity's failure to make accurate predictions, a different explanation for the relative stability of the postwar period is required. The offense-defense balance provides such an alternative.

Waltz argues that the postwar world has been stable due to bipolarity. In fact, bipolarity has had little to do with the absence of war between the Soviet Union and the United States. Waltz confounds polarity and strategic defense dominance.[18] The postwar international system has achieved the ultimate in strategic defensive advantage—the ability to destroy the other no matter what the other chooses to do. While Waltz does entertain the notion that strategic defense dominance is the real cause of postwar stability, he states his preference for a bipolar nuclear world over a tripolar system.[19]

Waltz argues that the instability that does exist in the world is a consequence of the poles' bias toward "overreaction" under bipolarity. He further argues that such instability is diminishing as a consequence of the "maturation" of the bipolar system (1979, 172, 203-4). However, the offense-defense balance provides an alternative to both "overreaction" and "maturation."

The primary impetus for military actions in the postwar world has been strategic beliefs, that is, concern for credibility and fears of falling dominoes and bandwagoning allies. These perceptions are widely and deeply documented, for U.S. decision makers and Soviet leaders expressed similar concerns over possible losses in Vietnam and the Middle East.[20]

Waltz might call this phenomenon "overreaction"; but he argues that this factor is powerful only under bipolarity, and that under multipolarity, miscalculation and uncertainty cause instability. But this "overreaction" characteristic of strategic beliefs is found in sixteenth-century multipolar Europe and Jack Snyder (n.d.) has found ample examples in nineteenth-century Britain, Wilhelmine Germany, and imperial Japan. If strategic beliefs are found to operate in favor of instability irrespective of the distribution of capabilities, Waltz's ascription of causality to both polarity and its consequences is unfounded.

The greatest limitation on instability in the postwar period, beyond strategic defense dominance, is not the "maturation" of the system but rather the rather low cumulativity of power resources. The power resources in this system are not at all like those of the sixteenth century or even those of the 1930s. Today, the economic power that undergirds a state's military capacity is most rapidly advanced through technological innovation and invention and through the exploitation of human capital and ever-finer gradations in the international division of labor. An illustrative example is the U.S. response to the enormous income losses suffered as a result of OPEC petroleum price increases. It is far less costly today to develop alternative sources of oil and to master alternative sources of energy than to occupy and administer oil-rich lands in the Middle East. Twenty-five years ago this might not have been true. In fact, the international political economic system in general is structured in such a way that it is virtually always cheaper to gain access to necessary raw materials and goods through international trade than through direct military intervention and occupation.[21]

Soviet size and relative autarky makes it a comparatively disengaged member of the international division of labor, which, in turn, inheres to the stability of the system. In this respect, size assumes a key role in preserving international stability. As Waltz argues (although from his conception of bipolarity), bipolar states increase their power through exploitation of domestic resources, rather than through external expansion.[22]

In sum, nuclear weapons that provide strategic defense dominance and the relatively noncumulative character of power resources explain postwar stability; bipolarity is irrelevant. Offensive strategic beliefs explain Soviet and U.S. conflict in peripheral areas of the globe; this set of beliefs, not polarity, is the root cause of "overreaction." If the offense-defense balance is the true cause of stability over the last forty-five years, we need not be so anxious about the post-Cold War world.[23] A multipolar world is nothing to fear; the defensive advantage conferred by nuclear weapons and the lack of cumulating power resources implies a continuation of postwar stability.

Notes

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1. The term *strategic beliefs* was developed by Jack Snyder to describe a similar set of assumptions. See Snyder n.d. For theoretical and empirical treatments of such beliefs, see the contributions by Stephen Walt, Deborah Larson, Robert Jervis, and Ted Hopf in Jervis and Snyder 1990. See also Van Evera 1985, 80-117.

2. See, for example, his critique of those who believe that military power has been supplanted as the final arbiter of the international system (Waltz 1979, 183-93).

3. The absence of reliable data on GNP in the period turns out to be a blessing in disguise, since the value of government revenues is a very robust, if indirect, measure of a state's military power in the period. Revenues reflect the ultimate extractive power of the state; whereas GNP could be, and was, distributed among the church, nobility, and other groups that were competing with the sovereign for extractive authority. Moreover, since armies were largely mercenaries in the period, how much money a state could raise is a very good measure of its military power.

4. On Maximilian I's military power, see Sorokin 1962, vol. 3 and Laffan 1957, 212-17. I have converted all the currencies in the period into Spanish gold ducats based primarily on Benecke 1974, 268, 297, 363; Braudel 1972, 2:420, 471, 698; Ehrenberg 1928, 17, 116, 180; Knecht 1984, 47, 107-8. I recognize the rough qualities of all these estimates but do not expect they will have any significant effect on their use as the very broadest comparisons. On Austrian finances, in particular, see Kellenbenz 1976, 180-85.

5. That the data in the table are only "average" army size understates the enormous power differential between the Habsburgs and Ottomans and all the other states in the system. When the *maximum* armed forces generated by the two poles is compared to the figures for the other powers, it becomes more clear just how deeply bipolar Europe was from 1521 to 1556.

6. Alvarez 1975, 97-159; Brandi 1939, 285, 316-24, 366, 460, 495-515, 548-52, 591, 621; Braudel 1972, 2:304, 419, 849-52, 930, 977; Chamberlain 1979, 92-96, 143; Hook 1972, 106-27, 242, 254; Merriman 1944, 209-28, 260-69; Oman 1937, 44-62, 84-98, 179-94, 219, 231-54, 277, 325-38, 657-64, 688-96; Parry 1958, 522-27.

7. The discussion of Habsburg finances draws primarily on Benecke 1974, 382-93; Chudoba 1952, 65-73; Ehrenberg 1928, 78-116, 148-73, 209; Fischer-Galati 1959, 14-92; Kellenbenz 1976, 38-41, 109, 172-86; Koenigsberger 1958, 314-30; Parker 1974, 527, 538, 561-68; Spooner 1958, 348-57; Wallerstein 1974, 180-91.

8. On Valois finances, see Knecht 1984, 49-55.

9. Cipolla 1976, 46-55, 167; Clay 1984, 142-45, 251-63; Hale 1958, 493-503; Oman 1937, 323-32, 351-69; Pickthorn 1934, 59-61, 382-93; Wernham 1971, 81-86, 101-13, 133-84, 200-211, 245, 279-80.

10. All statistics on population, GNP, size of army, and strategic forces are taken from International Institute for Strategic Studies 1989.

11. My discussion of the availability of power resources in this period relies on Ady 1957, 358; Ehrenberg 1928, 204; Fisher 1957, 34, 66; Mattingly 1955, 137, 182.

12. On the extractability of power resources, see Lockyer 1974, 209-10; Mallett and Hale 1984, 222-26; Oman 1937, 20; Parry 1957, 404; Petrie 1949, 16.

13. On the strategic offense-defense balance, see Brandi 1939, 367; Koenigsberger 1958, 325; Merriman 1944, 74, 104-6; Parry 1958, 519-27; Oman 1937, 220-21, 348, 675, 758.

14. In order to further invalidate polarity as a necessary cause of either stability and instability and validate the offense-defense balance and each of its three components, it is necessary to test the competing theories in other historical cases. Athens versus Sparta, Rome versus Carthage, and England versus France at the turn of the eighteenth century could provide these additional cases.

15. Brandi 1939, 398-408; Parry 1958, 517. It should be noted that religion was not an important cause of wars from 1495 to 1559 in the European system. It did not become a major factor until Phillip II's wars in the Netherlands in the last third of the century.

16. For another effort to use the structural variable of offense-defense to make determinate predictions about alliance dynamics under multipolarity, see Christensen and Snyder 1990.

17. On levels of economic interdependence, see Braudel 1972, 583-91; Clay 1984, 94-125, 209-18; Kortepeter 1972, 1-13; Rich and Wilson 1967, 158-71. I am grateful to Chaim Kaufmann for suggesting this argument to me.

18. My argument here takes issue with John Mueller's (1988) assertion that nuclear weapons have been irrelevant to keeping the postwar peace. For a rejoinder, see Jervis 1988, 1989. For a discussion of the possible relevance of both nuclear weapons and polarity to the "long peace," see Gaddis 1986.

19. Waltz 1979, 202. In fairness, it seems that Waltz has even further reconsidered his original arguments about polarity and nuclear weapons. He recently suggested that a multipolar world of the Soviet Union, United States, Germany, and Japan would not necessarily be unstable as long as the latter two countries were to acquire nuclear weapons. See Waltz 1990.

20. See, for example, the contributions of MacDonald, Larson, and Hopf in Jervis and Snyder 1990, and Hopf 1990, chaps. 2, 5.

21. On the changing nature of economic power resources, see Nye 1990 and Kaysen 1990, 49-57; Van Evera 1990/91.

22. Gilpin recognizes the extraordinary importance of Soviet disengagement from the international economic system as an obstacle on the path toward hegemonic war (1981, 239).

23. For an application of Waltz's systemic theory to the regional subsystem of Europe that generates grave apprehensions about post-Cold War stability, see Mearsheimer 1990.

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THE OFFICIAL LANGUAGE PROBLEM

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Polities and organizations use and require particular languages for official business. The choice of official languages is a vexing issue. Theorists, convinced that a fair language policy cannot be efficient, have despaired of an elegant solution. To investigate this apparent dilemma, I mathematically model the problem of choosing an efficient and fair language policy for a plurilingual polity. The policy designates official languages and taxes the language groups to pay for translation among the official languages. Contrary to prevailing wisdom, this model implies that a fair language policy can be efficient. But what if language groups rationally misrepresent the costs of using a nonnative official language? Even then, the policy maker can discover a fair language policy and, under some conditions, can use a cost-revelation procedure that discovers a fair and *efficient language policy. The results challenge the claim that efficiency and practicality excuse the inferior treatment of language minorities.*

In a world with thousands of languages, the choice of official languages is a natural political issue. When governments, firms, associations, and international organizations designate the languages that they require or permit to be used in official business, interests come into conflict. Those whose languages are not official spend years learning others' languages and may still communicate with difficulty, compete unequally for employment and participation, and suffer from minority or peripheral status.

The issue of official languages has often been described as one that resists peaceful, stable, or satisfying resolution. Possible explanations include the primordial, symbolic, divisive, uncompromisable character of language conflict (Rustow 1970); inherent incompatibilities between linguistic communities (Deutsch 1984; Laponce 1984); the reluctance of linguistic majorities to concede rights to minorities (Sniderman et al. 1989); the power of civil servants to protect their linguistic privileges (Laitin 1989; Mackey 1977); the important material and symbolic consequences of language policies (Weinstein 1983); and the difficulty of predicting these consequences (Fishman 1974, 92–94; Kelman 1971). As an example of this last claim, Laponce (1989) hypothesizes that if a government tries to fortify a language by making it official where it is weak, this act may backfire by drawing speakers away from the language's heartland, threatening the language's very survival. For these or other reasons, the choice of official languages is described in case after case as a frustrating one.[1]

Beyond the foregoing reasons, students of political and sociolinguistic theory may themselves deserve some blame for the recurring eruption of disputes over official languages. Theorists have failed to provide models for optimally resolving this issue. In fact, the theory of official languages echoes with pessimism as to whether any elegant solution could exist. A solution that treats all speakers of all languages identically is considered impractical. According to Kloss, "three seems to be the maximum number of languages which can be placed on an equal footing as official languages of the nation. The day-to-day affairs of a country's administration and even its legislative proceedings will soon be overtaxed, tangled, and inefficient if transacted in more than three languages" (1966, 7). Some claim it would be unnatural, inefficient, impossible, impractical, and contrary to common sense for governments to provide teachers and textbooks, even throughout elementary school, in all pupils' languages (Avrorin 1975, 205; Deseriev 1977, 259; and Guboglo 1979, 193). Van Dyke says governments have "no other practical choice" but to give different statuses to different languages; the idea "that claims for equal treatment in terms of language need to be balanced off against costs is a principle that all accept" (1976, 5–6). Green (1987), though arguing against any system of official languages that would diminish anyone's welfare, still assents to the officialization of the most powerful groups' languages. De Witte, while insisting that equality under the law would require governments to "use as many languages as are spoken by the target public," says services may be denied in some languages "on the basis of administrative or judicial efficiency. It is clear, for example, that a single user cannot demand an additional official language, but a group consisting of about half the population can legitimately do so" (1989, 97). Mazrui proposes five official "world languages," selected as a compromise among several principles, including "the sheer exigencies of power" (1976, 473–79).

Without a theoretical account of what an ideally democratic language policy would look like, it is no surprise that legislatures, courts, and regulators employ ad hoc numerical, cost, and political criteria when allocating official statuses to languages (Anderson and Silver 1984; Falch 1973). Examples include guarantees of minority-language schools where "a considerable proportion of Polish nationals of other than Polish speech are resident" (Laqueur and Rubin 1979, 154–55), government services in both English and French where there is a "significant demand" (Canada, Commissioner of Official Languages 1989, 34), public notices in the language of any nationality constituting at least 30% of a jurisdiction (Falch 1973, 89–90), emergency telephone operator service in languages spoken by at least 5% of a local population (Rubin 1984, 162), and voting instructions in languages of poorly educated groups constituting at least 5% of a state or local population (42 U.S.C. §1973aa-1a). U.S. Supreme Court justices Burger and Blackmun agreed that eighteen hundred children had a right to recognition of their language by a public school district but said they might reject a similar claim "when, in another case, we are concerned with a very few youngsters, or with just a single child" (*Lau* versus *Nichols* 1974, 572).

Although the intrinsic inelegance of the official language problem is variously described, most theorists appear to have concluded that the choice of official languages involves an inevitable compromise between efficiency and fairness. It is usually claimed that an efficient language policy officializes fewer than all languages and is therefore unfair, while a fair policy officializes everyone's language or an entirely alien language and is therefore inefficient. Efficient neutrality, exemplified in church-state separation and racial nondiscrimination, is held inapplicable to language groups, because governments can simply *ignore* races and religions, but must *use*, and thus *choose*, languages.

A principled solution to the official language problem seems to be further inhibited by the large number of norms that theorists (e.g., Fishman 1973, 63–71; Pool

1976; Thorburn 1971; Vaillancourt 1983; Vikør 1988, 96–138; Woolard 1989) acknowledge in addition to efficiency and fairness. These include authenticity (favoring indigenous languages), uniformity (favoring only one language), diversity (favoring multiple languages), distinctiveness (favoring languages unique to the community), universality (favoring languages known by outsiders), stability (favoring existing language rights and statuses), radicality (using language policy to liberate oppressed groups), definitiveness (avoiding linguistic options), liberty (noncoercion), modernization (favoring languages with developed lexicons and literatures), populism (favoring mass over elite languages), prestige (recognizing already-high-status languages), antibossism (discouraging powerful linguistic intermediaries), and tolerability (avoiding policies that would induce emigration or secession).

A few voices have suggested that efficiency and fairness in a policy on official languages can be reconciled, but their ideas remain to be explored. Among the proposed solutions are voluntarism (Isaev 1977, 21–23, 337–38, 351), compensation (Pool 1987), democratization (O'Barr 1976, 19), and even linguicide—the obliteration of all native languages except a single official language (Atkins 1978). Each of these ideas, even if superficially plausible, is problematic; and no one has yet rigorously defined and analyzed any of them.

The prevailing belief that an efficiency-fairness conflict in official language policy exists and that it justifies evidently unfair policies merits scrutiny. In a world that largely embraces democratic norms, one might expect defenses of unfair policies to be held to a high standard of proof, particularly when, as in this arena, the policies substantially affect the lives of billions of persons. Nevertheless, the case for allowing official language policies to be unfair has been made with the support of only impressionistic arguments.

I plan to show that fairness is compatible with efficiency in a policy on official languages. I present a formal model of the official language problem defining policy alternatives, their consequences, and normative principles that one might wish to satisfy in choosing a language policy. I then prove that there are language policies simultaneously satisfying the fairness and efficiency principles and that procedures exist by which one can discover such policies, even when the affected groups have private information about the policy consequences and are motivated to conceal that information from the policy-making authorities. Finally, I summarize the results, discuss their robustness, and suggest possible elaborations of the model.

Model

What, in essence, is the official language problem? Abstracting from the intricate complexity and variety of practical language politics, I here define the official language problem as a set of language policy choices that have particular consequences and that are subject to particular normative criteria. These choices, consequences, and criteria are expressed in a model consisting of 10 formal assumptions, which I elucidate with accompanying motivations.

ASSUMPTION 1. *A polity is partitioned into two or more groups, each with a positive size and a different native language, drawn from a finite set of languages.*

This assumption reflects the fact that almost all persons can be described as having one native or first language. For simplicity, I assume no one is natively plurilingual. Each group can be understood as the set of citizens sharing some native language, with the group size being proportional to the number of citizens in it. The assumptions below do not introduce any within-group differences. Accordingly, I shall not analytically disaggregate the groups into individuals in this model.

ASSUMPTION 2. *At least one language is official.*

Each state or other organization generally officializes at least one language, if not de jure then de facto. Assumption 2 allows languages not native to any group in the polity to be official, reflecting the fact that foreign, artificial, classical, and pidgin languages are sometimes given official status. While various kinds and degrees of official status are granted to languages in practice, I simplify by assuming only that every language is either official or not.

ASSUMPTION 3. *For each group whose native language is not official, each official language has a positive adoption price that depends only on the group and the language.*

Typically, persons whose native languages are not official adopt some official language for use in communicating with the rest of the polity. The cost of adopting an official language might consist of time, effort, and money spent in learning the language; deprivations caused by imperfect command of the language; and the loss of prestige arising from the denial of official status to one's native language. All such effects can be viewed as an aggregated price for adopting a particular official language. I assume the adoption price of a particular official language for a particular group doesn't change when any other language is officialized or deofficialized. Thus, the adoption price of official language I for group J is not affected by whether language K, too, is official.

ASSUMPTION 4. *The adoption cost of each group is zero if its native language is official and otherwise is the minimum of the adoption prices of all official languages for the group.*

If a group's own language is official, it typically uses that language and incurs no cost in adapting to an official language. Otherwise, it would be reasonable to expect the group to choose the official language that is least costly for the group.

ASSUMPTION 5. *The translation cost of each group is the product of the group's size and one less than the number of official languages.*

Suppose each group knows and uses only one official language. Suppose, further, there are no superfluous official languages (i.e., official languages that no group uses). Suppose, finally, the polity is participatorily egalitarian: each citizen communicates to the same extent; in terms of assumption 1, the volume of each group's public utterances is proportional to the group's size. Under these conditions, how can all groups understand one another? Each public utterance of any group must be translated into each of the official languages other than the one in which it is uttered. Thus, if o is the number of official languages, each utterance must be translated $o - 1$ times. And what does it cost to translate all the public utterances of a group (its translation cost)? Presumably, the cost is proportional to the volume of those utterances and hence to the group's size, and also proportional to the number of times they are translated $(o - 1)$. The cost then equals some constant multiplied by group size and $o - 1$. Assumption 5 adopts a measurement scale that makes this constant 1.

ASSUMPTION 6. *The total cost is the sum of the groups' adoption costs and translation costs.*

For any set of official languages, any group has an adoption cost and a translation cost. The total of all these costs for all groups is the total cost that a set of official languages imposes on the polity.

ASSUMPTION 7. *The tax schedule is a set of taxes on the groups, the sum of the taxes being equal to the sum of the translation costs.*

Suppose the state performs the translation and does nothing else. Then it might also redistribute wealth by taxing the groups disproportionally to their sizes. Tax rates sometimes in fact differ among language groups. The differences may be by-products of transfers to poorer groups or regions (e.g., in Yugoslavia and Canada) and wealth-based assessments (e.g., in the United Nations) or may reflect an overtly language-based subsidy, such as what the Swiss government pays to the Romansh- and Italian-speaking regions (McRae 1983, 169–72).

ASSUMPTION 8. *The language policy is the set of official languages and the tax schedule. A language regime is a rule producing a language policy. A language regime is* based on *a set of facts if it does not refer to any facts not in the set.*

Both a set of official languages and a tax schedule (which may tax the native speakers of different languages at different rates) can be regarded as ingredients of a language policy. The language policy may be determined by some rule, such as a constitutional provision. Such a rule would be a function: facts (such as the number and sizes of the groups) would be input, and a language policy would be output. I call such a rule a *language regime.*

ASSUMPTION 9. *A language policy is efficient if it minimizes the total cost. A group's burden is the sum of its adoption cost and its tax. A language policy is fair if each group's burden is proportional to the group's size.*

Since this model provides a measure of total social cost, and since assumption 5 guarantees that the total social benefit (communicability) will be constant regardless of the language policy that is chosen, a reasonable notion of efficiency for this model is the minimization of the total cost. As for fairness, any language policy involves an adoption cost and a tax (or subsidy) for each group; I call their sum the group's *burden.* Suppose each group's burden is shared uniformly by its members. Then individualistic fairness would require that each group's burden be proportional to the group's size.

ASSUMPTION 10. *A language regime is objective if it is based on the number of groups, their sizes, and their native languages and semiobjective if based on these and on groups' choices.*

If one wants an efficient and fair language policy, it is reasonable to want a language regime—a rule—that guarantees such a policy. A language regime based on the number of groups, their sizes, and their native languages is especially appealing. The reason is that these facts are relatively easy to measure. Excluded from this set of facts are adoption prices. As discussed after assumption 3, adoption prices may include subjective as well as observable elements. If a language regime measured adoption prices by asking groups to disclose them, the replies might be false. Groups might want to mislead the language regime into officializing their native languages and/or lowering their taxes. This problem is an instance of the general principle that telling the truth is not always rational if a policy that affects the teller is going to be based on the teller's disclosure (see Dasgupta, Hammond, and Maskin 1979; Radner 1986; Rasmusen 1989, 133–36, 172–75). Accordingly, I define a language regime as *objective* if it doesn't require as an input any adoption price. But what if we can't guarantee an efficient and fair language policy without considering adoption prices? Then we might seek to induce the groups to make choices that will reveal adoption prices. I call a language regime that relies on such choices *semiobjective.* The language regime can use such choices to infer adoption prices, but its inferences may be incorrect.

Results, Proofs, and Discussion

Under the foregoing model, can a language policy be efficient and fair? If so, is there an objective way to produce such a policy? I shall present six results, followed by their proofs and discussions. Readers wishing to skip the proofs can do so without loss of continuity.

RESULT 1. *An efficient language policy always exists.*

Proof. For any polity, each language policy has a total cost. Some language policy's total cost is the smallest. Any language policy with this total cost (at least one must have it) is efficient.

RESULT 2. *A fair language policy always exists.*

Proof. Consider a language policy with all groups' native languages official and each group's tax equal to the group's own translation cost. Assumption 7 requires that the sum of the taxes be the sum of the translation costs, and this is the case for this language policy. Because each group's native language is official, each group's adoption cost is zero. Thus, each group's burden is its tax. By assumption 5, translation costs are proportional to size, so with this policy taxes are proportional to size; thus, burdens are proportional to size. By assumption 9, that makes this policy fair.

Results 1 and 2 are consistent with previous commentaries, which generally imply that a language policy can be efficient or fair. The consensus denies, however, that it can be both efficient and fair.

RESULT 3. *An efficient, fair language policy always exists.*

Proof. We can take any efficient language policy (at least one always exists, by result 1) and make it fair by changing its tax schedule. A changed tax schedule will leave the total cost unchanged, so the new language policy will remain efficient, while now also being fair. For the new tax schedule, I shall identify the groups as group 1, group 2, and so on and shall make o the number of official languages, s_i the size of group i, a_i the adoption cost of group i, t_i the translation cost of group i, and x_i the tax on group i. I shall use s, a, t, and x for the respective whole-polity sums, and we know from assumption 7 that $x = t$. Assumption 5 says that $t_i = (o - 1)s_i$. Summing the equations for all t_i, we get the total translation cost: $t = (o - 1)s$. To make a language policy fair, we must change its tax schedule so each group's burden is proportional to its size. Each group's burden must then bear the same ratio to the total burden as the group's size bears to the total size. By assumption 9, group i's burden is $a_i + x_i$, so

$$\frac{a_i + x_i}{a + x} = \frac{s_i}{s}. \tag{1}$$

Solving equation 1 for x_i, we find that each group's tax is

$$\begin{aligned} x_i &= \frac{a + x}{s} s_i - a_i = \frac{a + t}{s} s_i - a_i \\ &= \frac{a + (o - 1)s}{s} s_i - a_i \\ &= (a/s + o - 1)s_i - a_i. \end{aligned} \tag{2}$$

Efficiency and fairness, as defined here, are compatible because they constrain different things. Efficiency requires minimizing the total cost, and the total cost depends solely on the set of official languages. Fairness requires proportional burdens, and this can be achieved with an appropriate tax schedule, regardless of the set of official languages.

Result 3 challenges the common claim that efficiency justifies the unequal treatment of different languages' speakers. In this model efficiency is no barrier to fairness. The purported efficiency-fairness conflict results from certain (usually unstated) assumptions. It fails to arise under my assumptions.

For examples of how the model can prescribe an efficient and fair language policy, imagine these "facts" about a polity:

1. each person's lifetime earning potential (which will be the unit of value) is equal;
2. each person spends 1% of a working life using an official language to communicate to the polity;
3. a translator spends the same time translating an utterance as it took to utter;
4. a person whose native language is not official spends 5% of a working life (2 out of 40 years) learning an official language; and
5. its members' learning time constitutes a group's adoption cost.

On these "facts," efficiency would require officializing the largest native language and also any other language natively spoken by more than 20% of the population. If we let l be the native speakers of official languages as a proportion of the population, the fair per capita tax would be $.01o - .05l + .04$ on official language speakers and $.01o - .05l - .01$ on others. The three terms imply that the tax would (1) vary directly with the number of official languages, (2) vary inversely with the fraction of the population not natively speaking an official language, and (3) exactly compensate those who must learn an official language. Official languages and per capita taxes would be insensitive to polity size. Table 1 compares, for several countries, two fair language policies, the efficient one and a plausible but inefficient one, assuming the named "facts." Note that because the taxes are fair, *all* persons—even those whose native languages lose official status—prefer the efficient policy to the inefficient one. The German-speaking Belgians' case in Table 1 will illustrate this principle. Belgium's native German speakers pay a negative tax (receive a subsidy) of 3.61% of their income if only Dutch (A) and French (B) are official but pay a tax of 2.34% of their income if all three languages are official. The difference is 5.95% of their income. But it costs them only 5% of their income to learn Dutch or French. So they prefer the policy that officializes only Dutch and French.

Anyone familiar with any of Table 1's cases will recognize serious complications in the politics of official languages that are ignored in the model and hence in results 1–3. For example, language groups tend to be (as in Belgium) geographically concentrated, making regional officialization policies (as in Belgium) plausible. There are many other complications (such as intergenerational assimilation, variable translation cost rates, and variable participation) that await more elaborate models, as I shall discuss in the conclusion.

In excluding most such complications from this model, I am not diminishing their importance. I exclude them because each is important enough (and probably tricky enough) to deserve a detailed study of its own.

I shall, however, select a single important complication for analysis: cost measurement. I have shown that an efficient and fair language policy exists, but I have not shown that we can find it. One obstacle, mentioned above in my motivation for assumption 10, is the subjectivity of adoption prices. The language regime described for result 3 is based in part on adoption prices. Suppose we refuse to posit adoption prices, such as those posited by the fourth and fifth "facts" in the last example. Suppose, further, we refuse to assume that groups will sincerely reveal adoption prices when asked. Can we design a language regime that infallibly produces an efficient, fair language policy objectively (i.e., without information about adoption prices)?

RESULT 4. *At least one objective language regime always produces a fair language policy.*

Proof. In proving result 2, I described a language regime that always produces a fair language policy, namely, to make all groups' languages official and make each

Table 1. Alternative Fair Language Policies for Several Countries

Country	Native Speakers by Language (%)	Official Languages	Tax on Speakers[a] (% of Total Income)	Subsidy to Others	Efficient?
Belgium	Dutch (59)	Dutch, French	1.39	3.61	yes
	French (33)	all	2.34	2.66	no
	German (1)				
Canada	English (60)	both	1.76	3.24	yes
	French (24)	English	1.98	3.02	no
India	Bengali (8)				
	Hindi (28)	Hindi	3.60	1.40	yes
	Marāthi (8)	all	6.06	-1.06	no
	Tamil (7)				
	Telugu (8)				
Peru	Quechua (27)	both	1.27	3.73	yes
	Spanish (68)	Spanish	1.60	3.40	no
Philippines	Cebuano (24)				
	Hiligaynon (10)	Cebuano, Pilipino	3.59	1.41	yes
	Ilocano (11)	all	4.53	.47	no
	Pilipino (24)				
South Africa	Afrikaans (16)	Zulu	3.95	1.05	yes
	Xhosa (18)	all	4.24	.76	no
	Zulu (21)				
USSR	Russian (59)	Russian	2.07	2.93	yes
	Ukrainian (13)	all	3.17	1.83	no
	Uzbek (5)				
United States (1990)	English (89)	English	.57	4.43	yes
	Spanish (6)	both	1.29	3.71	no
United States (2080)	English (74)	both	1.27	3.73	yes
	Spanish (21)	English	1.30	3.70	no

Sources: 1990 *Britannica Book of the Year,* pp. 762–67, 774–77; Spencer 1986, 1, 10; Strategy Research Corporation 1987, 176. U.S. forecasts for 2080 assume Hispanics' current language composition and Puerto Rico statehood.

Note: Main assumptions: translation costs 1% of total income per target language; nonnative speakers learn one official language at cost of 5% of total income.

[a] Native speakers of any official language.

group's tax equal to its translation cost. With n groups, group i's tax is then $x_i = t_i = (o - 1)s_i = (n - 1)s_i$. Thus, the language regime is based only on the number of groups (n), their sizes (s_i), and their native languages. By assumption 10, this makes it an objective language regime.

If all the native languages are official, every group's adoption cost is zero, and one can make the language policy fair without ascertaining adoption prices. Result 4 further challenges defenses of unfair language policies. Objectivity, like efficiency, is no bar to fairness in this model.

RESULT 5. *There is no objective language regime that always produces an efficient language policy.*

Proof. Consider a two-group polity with the native languages as the only languages. Let us make L the set of official languages, p_i the adoption price for group i of its nonnative language if its native language is not official, and c the total cost. Then L can be {1}, {2}, or {1, 2}. With {1}, the only cost is an adoption cost, p_2. With {2}, the only cost is an adoption cost, p_1. With {1, 2}, the only cost is the translation cost, $(o - 1)s$, which in this example, with $o = 2$, is s. Thus,

$$c = \begin{cases} p_2 \text{ if } L = \{1\} \\ p_1 \text{ if } L = \{2\} \\ s \text{ if } L = \{1, 2\}. \end{cases} \quad (3)$$

To know that a language policy is efficient in this case requires knowing whether p_2, p_1, or s is smallest. But no objective language regime has access to p_1 or p_2. Thus, no objective language regime can guarantee an efficient language policy in this case or, consequently, in all cases.

An objective language regime may identify some inefficient language policies, but it can't identify all of them. For example, any policy that officializes all the groups' native languages and at least one other language must be inefficient. But policies that officialize only groups' native languages can't be classified as efficient or inefficient without a knowledge of adoption prices.

Under this model's assumptions, it is wrong to claim (as is often done) that having many official languages is necessarily inefficient. As more native languages are made official, translation costs rise but adoption costs fall. If all adoption prices are sufficiently large, it will be efficient to officialize all the groups' native languages. Why, then, do people assert that official multilingualism is inherently inefficient? They may assume that the cost of translation is efficiency-relevant but the cost of language adoption is not. In my model translation is paid for centrally, while language adoption is paid for by the adopters; and in many cases the facts seem to resemble this assumption. The tendency to regard multiple official languages as inefficient may, then, reflect a state-centered neglect of costs incurred by individuals in adapting to language policies.

The search for an objective language regime that reliably produces an efficient and fair language policy has failed. Such a regime can guarantee a fair but not an efficient language policy. I appeal for help, then, to *semi*objective language regimes; and, at least under some conditions, they succeed. In specifying the conditions of success, I shall assume that a semiobjective language regime may force any group, in any sequence, to choose among any set of alternatives and may also *recommend* to any group a principle for making a given choice. I shall further assume that each group follows some *choice strategy,* which determines how the group chooses among any set of alternatives under given conditions. For the sake of analytical simplicity, I now define two special attributes that groups may have. A group is *completely informed* if it knows all relevant facts, including all adoption prices, all previously made choices, all facts about the language regime, and all facts about each group's choice strategy. A group is *deferentially rational* if its choice strategy is to make a choice that minimizes the group's expected burden and, whenever two or more alternatives would be equally burden-minimizing, to choose among those alternatives according to whatever principle is recommended to the group by the language regime.

RESULT 6. *In a polity with two languages and two completely informed, deferentially rational groups, at least one semiobjective language regime always produces an efficient, fair language policy.*

Proof. My proof has six parts. In part 1, I define a semiobjective language regime that obtains sequential choices from groups. In part 2, I show the language policy that the regime produces, given any sequence of group choices. In parts 3–5, I show the choices that the groups make. In part 6, I show that the language policy is always efficient and fair.

Part 1. The *suspicious own-price-soliciting* (SOPS) language regime is defined by steps A–H:

Choice solicitation

A. The groups are arbitrarily numbered 1 and 2.
B. Group 1 is instructed to choose a positive number, and it is recommended that this number be the adoption price for group 1 of language 2.
C. Next, group 2 is instructed to choose a positive number, and it is recommended that this number be the adoption price for group 2 of language 1.
D. Next, group 2 is instructed to choose the word *one* or *two*, and it is recommended that this word be *one*.
E. Next, if the number chosen by group 1 (step B) is greater than the number chosen by group 2 (step C) and no greater than s, group 1's choice is discarded and group 1 is again instructed to choose a positive number, and it is again recommended that this number be the adoption price for group 1 of language 2.
F. The *primary recommendations* made in steps B, C, and E are supplemented by the *subordinate recommendation* that the group choose the largest of the numbers that it considers.

Policy production

G. A language policy is produced, and it is defined so as to be efficient and fair if the number last chosen by each group is the adoption price for that group of its nonnative language.
H. If step G fails to specify a unique set of official languages, {1} or {2} is officialized in preference to {1, 2}, and if {1} and {2} are tied the word chosen in step D names the official language.

To refer to elements of this language regime, I shall use the following terms: a *claim* is a choice made in step B, C, or E; a claim is *true* if it obeys the primary recommendation; a *disclosure* is a nondiscarded claim; the choice made in step D and the disclosures are *signals;* an *apparent* attribute of the language policy is an attribute that it possesses when both disclosures are true. I shall also make q the initial claim of group 1, r the revised claim (if any) of group 1, d_i the disclosure of group i, c' the apparent total cost, a'_i the apparent adoption cost of group i, and a' the sum of the apparent adoption costs.

Part 2. Given any combination of signals, the SOPS language regime produces an apparently efficient language policy. In other words, the language policy is efficient if both disclosures are true but may or may not be efficient otherwise. Thus, the policy minimizes the apparent total cost c', given by

Table 2. Outcomes in a Two-Language Polity under a Suspicious Own-Price-Soliciting Language Regime

Situation		L	Group	a'_i	a_i	Tax on Group i (x_i)	Group i Burden $(a_i + x_i)$
1.	$\{d_2 < d_1, d_2 \leq s\}$ or $\{d_1 = d_2 \leq s, one\}$	$\{1\}$	1	0	0	$(s_1/s)d_2$	$(s_1/s)d_2$
			2	d_2	p_2	$(-s_1/s)d_2$	$p_2 - (s_1/s)d_2$
2.	$\{d_1 < d_2, d_1 \leq s\}$ or $\{d_1 = d_2 \leq s, two\}$	$\{2\}$	1	d_1	p_1	$(-s_2/s)d_1$	$p_1 - (s_2/s)d_1$
			2	0	0	$(s_2/s)d_1$	$(s_2/s)d_1$
3.	$\{d_1 > s, d_2 > s\}$	$\{1, 2\}$	1	0	0	s_1	s_1
			2	0	0	s_2	s_2

$$c' = \begin{cases} d_2 \text{ if } L = \{1\} \\ d_1 \text{ if } L = \{2\} \\ s \text{ if } L = \{1, 2\}. \end{cases} \qquad (4)$$

Equation 4 is equation 3 with the disclosures substituted for the respective adoption prices. Whichever of $\{d_1, d_2, s\}$ is smallest, the regime makes the corresponding language(s) official. Any ties are broken as described in step H.

The SOPS language regime also produces an apparently fair language policy. Thus, the apparent burdens, $a'_i + x_i$, are proportional to group sizes. Adapting equation 2, we find that the *apparently* fair tax on group *i* is

$$x_i = (a'/s + o - 1)s_i - a'_i.$$

A complete description of the language policies produced by the SOPS language regime appears in Table 2. It classifies all the possible combinations of signals into three mutually exclusive situations. In each situation, the language regime officializes a different set of languages. For each situation, Table 2 specifies the taxes that will be imposed on the groups. The burdens shown in Table 2 are the true—not apparent—burdens that result from the language policies. These burdens will be used in later parts of the proof.

As an illustration of how Table 2 is derived, consider what happens if group 2's disclosure, d_2, is smaller than group 1's disclosure, d_1, and no greater than s. The language regime then officializes $\{1\}$ (according to equation 4). The taxes it imposes are determined according to equation 5. For example, the tax on group 2 is derived as follows:

$$\begin{aligned} x_2 &= (d_2/s + 1 - 1)s_2 - d_2 \\ &= (d_2/s)(s - s_1) - d_2 \\ &= d_2 - \frac{s_1}{s} d_2 - d_2 = \frac{-s_1}{s} d_2. \end{aligned}$$

Part 3. If group 1's claim is resolicited, group 1 chooses a revised claim that will minimize group 1's burden. In determining how its revised claim (r, which then becomes d_1) will affect its burden, group 1 is completely informed. Thus, it knows how its burden will depend on d_1, as shown in Table 2.

A complete description of the revised claims made by group 1 appears in Table 3. This table applies under the condition described in step E, namely when $d_2 < q \leq s$. When this condition is met, then, depending on group 2's prior choices, group 1 may be in any of three mutually exclusive positions. For each position, Table 3 specifies group 1's revised claim, together with the situation and burdens resulting from that claim.

It is a straightforward matter to derive r in Table 3 from Table 2. For illustration,

Table 3. Group 1's Revised Claim in a Two-Language Polity under a Suspicious Own-Price-Soliciting Language Regime

Position	r	Situation	Group 1 Burden	Group 2 Burden
I. $p_1 > d_2$ or $\{p_1 = d_2$, *one*$\}$	p_1	1	$(s_1/s)d_2$	$p_2 - (s_1/s)d_2$
II. $\{p_1 \leq d_2$, *two*$\}$	d_2	2	$p_1 - (s_2/s)d_2$	$(s_2/s)d_2$
III. $\{p_1 < d_2$, *one*$\}$	d_2^-	3	$p_1 - (s_2/s)d_2^-$	$(s_2/s)d_2^-$

Note: $^-$ designates a quantity infinitesimally smaller than the quantity to which it is appended.

suppose $d_2 < q \leq s$ and $d_2 < p_1$, and suppose the word is *one*. The former inequality causes group 1's claim to be resolicited. The latter inequality puts group 1 into position I. Table 3 says that group 1's rational revised claim is then true ($r = p_1$), leading to situation 1 and giving group 1 a burden of $(s_1/s)d_2$. Situation 1 emerges from this revised claim because $d_1 = r = p_1 > d_2 < s$, and these inequalities satisfy the definition of situation 1 in Table 2. Now consider why it is rational for group 1 to tell the truth in position I. Suppose group 1 lied by making $p_1 \neq r \geq d_2$. Then situation 1 would still emerge (because the definition of that situation in Table 2 would remain satisfied), and group 1's burden would not change (because it is not a function of r). Now suppose group 1 lied by making $p_1 \neq r < d_2$. Then situation 2 would emerge (because $\{d_1 < d_2, d_1 \leq s\}$ would be satisfied), and group 1's burden would become $p_1 - (s_2/s)d_1$. This burden would be an increase, as shown by

$$p_1 - \frac{s_2}{s} d_1 > d_2 - \frac{s_2}{s} d_1 > d_2$$

$$- \frac{s_2}{s} d_2 = (1 - s_2/s)d_2$$

$$= \frac{s - s_2}{s} d_2 = \frac{s_1}{s} d_2.$$

Thus, under the conditions of this illustration group 1 minimizes its burden by telling the truth. It would equally minimize its burden by telling some lies; but it is deferentially rational, so when breaking ties, it tells the truth, obeying the primary recommendation in step E. Similar arguments lead to the conclusions of Table 3 about all other possible cases in which group 1's claim is resolicited.

Part 4. Group 2 knows how its burden will depend on the choices it makes. Thus, it knows that if $q > s$ no choices by group 2 can cause group 1's claim to be resolicited, but that if $q \leq s$ group 2 can cause the resolicitation of group 1's claim by making $d_2 < q$. In either case, group 2 can use Tables 2 and 3 to predict the effect of its choices on its own burden. Given this complete information, group 2's choices are shown in Table 4.

The possible initial claims of group 1 can put group 2 into any of the five mutually exclusive cases in Table 4. Since it is not immediately obvious that these cases are exhaustive and mutually exclusive, a more transparent but logically equivalent diagram of the cases is presented in Figure 1. For each case, Table 4 shows group 2's signals, including its disclosure (d_2) and the word it chooses. The table also shows the position into which these signals put group 1 (in Table 3), if relevant, and the situation and burdens that result.

To verify Table 4's predictions, one can use Tables 2 and 3 to determine whether group 2 could obtain a smaller burden by giving any signal other than the predicted one. I shall summarize this verification for

Table 4. Group 2's Signal in a Two-Language Polity under a Suspicious Own-Price-Soliciting Language Regime

Case	d_2	Word	Position	Situation	Group 1 Burden $(a_1 + x_1)$	Group 2 Burden $(a_2 + x_2)$
1. $\{p_1 < q \le s,\ p_2 > (s_1q + s_2p_1)/s\}$	p_1	*two*	II	2	$(s_1/s)p_1$	$(s_2/s)p_1$
2. $\{p_2 \le q \le s,\ p_2 \le (s_1q + s_2p_1)/s\}$	q	*one*	—	1	$(s_1/s)q$	$p_2 - (s_1/s)q$
3. $p_2 \le s < q$	s	*one*	—	1	s_1	$p_2 - s_1$
4. $\{q < p_2,\ q \le s,\ q \le p_1\}$	p_2	*one*	—	2	$p_1 - (s_2/s)q$	$(s_2/s)q$
5. $\{s < p_2,\ s < q\}$	p_2	*one*	—	3	s_1	s_2

case 1. In case 1, Table 4 predicts that group 2 will choose the word *two* and also that group 2's disclosure will be a lie. Instead of truthfully making $d_2 = p_2$, group 2 will falsely make $d_2 = p_1$. This disclosure must be false, because $p_2 > p_1$, as shown by

$$p_2 > \frac{s_1q + s_2p_1}{s} > \frac{s_1p_1 + s_2p_1}{s} = \frac{sp_1}{s} = p_1.$$

When we consider group 2's alternative signals, we find that they all increase group 2's burden, compared with the predicted signals. Suppose group 2 made $d_2 \ge q$. This would directly produce situation 2, and group 2's burden would increase from $(s_2/s)p_1$ to $(s_2/s)q$. Suppose group 2 made $p_1 < d_2 < q$. This would leave group 1 still in position II, but group 2's resulting burden would be $(s_2/s)d_2$, again greater than before. Suppose group 2 made $d_2 < p_1$. This would move group 1 into position I and give group 2 a burden of $p_2 - (s_1/s)d_2$. This would be an increased burden, as shown by

$$\begin{aligned} p_2 - \frac{s_1}{s} d_2 &> p_2 - \frac{s_1}{s} p_1 \\ &> \frac{s_1q + s_2p_1}{s} - \frac{s_1}{s} p_1 \\ &> \frac{s_1p_1 + s_2p_1}{s} - \frac{s_1}{s} p_1 \\ &= p_1 - \frac{s_1}{s} p_1 = (1 - s_1/s)p_1 \\ &= (s_2/s)p_1. \end{aligned} \tag{5}$$

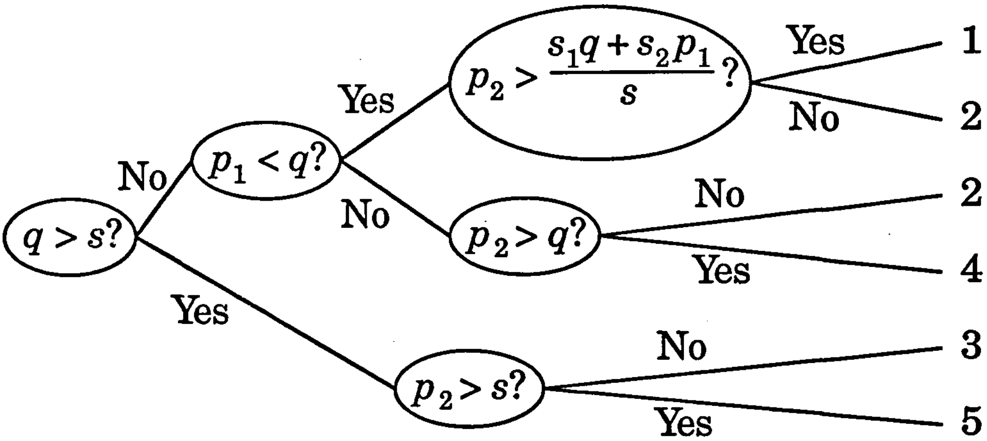
Now suppose group 2 changed *two* to *one* and kept $d_2 = p_1$. This would move group 1 into position I and make group 2's burden $p_2 - (s_1/s)p_1$, which inequality 5 shows would be an increase. Changing *two* to *one* and making $d_2 = q$ would directly produce situation 1 and give group 2 a burden of $p_2 - (s_1/s)q$, which would also be an increase because

$$p_2 - \frac{s_1}{s} q > \frac{s_1q + s_2p_1}{s} - \frac{s_1}{s} q = \frac{s_1}{s} q + \frac{s_2}{s} p_1 - \frac{s_1}{s} q = \frac{s_2}{s} p_1.$$

Changing *two* to *one* and making $p_1 < d_2 < q$ would move group 1 into position III and give group 2 a burden of $(s_2/s)d_2$, an obvious increase over $(s_2/s)p_1$. Changing *two* to *one* and changing d_2 in any other way would have the same effects as the corresponding changes of d_2 without changing the word. Thus, every possible pair of signals by group 2 other than $d_2 = p_1$ and the word *two* would increase group 2's burden, making these signals uniquely burden-minimizing. Similar reasoning gives the conclusions of Table 4 for cases 2–5.

Part 5. The initial choice by group 1 anticipates the responses shown in Table

Figure 1. Alternative Representation of Case Definitions in Table 4



4. The relative magnitudes of p_1, p_2, and s can put group 1 into any of the four mutually exclusive states, shown in Table 5, which also shows for each state the choice (q) that group 1 makes and the resulting set of official languages, burdens, and total cost (c).

As in parts 2–4, I shall demonstrate the rationality of the predicted choice for a single example, this one defined by $\{p_2 \leq p_1, p_2 < s\}$. Under these conditions, group 1 is in state A, and Table 5 predicts that group 1's disclosure will be a lie, with $q = p_2$ instead of $q = p_1$. This disclosure puts group 2 into case 2 (in Table 4) and makes group 1's burden $(s_1/s)p_2$. If, instead, group 1 made $q > s$, group 2 would be moved into case 3, and group 1's burden would change to s_1, which would be an increase, since $p_2 < s$. If group 1 made $p_2 < q \leq s$, group 2 would still be in case 2, but group 1's burden would increase to $(s_1/s)q$. Finally, making $q < p_2$ would move group 2 into case 4 and give group 1 a burden of $p_1 - (s_2/s)q$. This would be an increased burden, since

$$p_1 - \frac{s_1}{s} q > p_2 - \frac{s_2}{s} p_2$$

$$= (1 - s_2/s)p_2 = \frac{s_1}{s} p_2.$$

Thus, all of the possible alternatives to the predicted choice would increase the burden on group 1.

Part 6. Through parts 2–5 we know, for any possible set $\{p_1, p_2, s\}$, what choices the groups will make and what policy the language regime will produce. Table 5 shows that the language policy is always efficient and fair. The set of official languages achieves a total cost, c, equal to the smallest of $\{p_1, p_2, s\}$, making the language policy efficient. And in each of the four states the groups' burdens are proportional to their sizes (s_1 and s_2), making the policy fair. The proof is now complete.

At least under some conditions (specifically, when there are two completely informed and deferentially rational groups and two languages) a semiobjective language regime can produce a language policy that is guaranteed to be both efficient and fair. The language regime uses choices by the groups to infer adoption prices and bases its language policy on these inferences.

Table 5. Group 1's Signal in a Two-Language Polity under a Suspicious Own-Price-Soliciting Language Regime

State	q	Case	L	Group 1 Burden $(a_1 + x_1)$	Group 2 Burden $(a_2 + x_2)$	c
A. $\{p_2 \leq p_1, p_2 < s\}$ or $p_1 = p_2 = s$	p_2	2	$\{1\}$	$(s_1/s)p_2$	$(s_2/s)p_2$	p_2
B. $s = p_2 < p_1$	p_1	3	$\{1\}$	s_1	s_2	p_2
C. $\{p_1 < p_2, p_1 \leq s\}$	p_1	4	$\{2\}$	$(s_1/s)p_1$	$(s_2/s)p_1$	p_1
D. $s < p_1, s < p_2\}$	p_1	5	$\{1, 2\}$	s_1	s_2	s

A semiobjective language regime that achieves efficiency and fairness is valuable because, by assumption, the policy maker can't observe true adoption prices. Superficially, it might seem artificial to deny this information to the policy maker when result 6 assumes the groups know one another's adoption prices. But it would be naive to suppose that mere knowledge of true adoption prices by policy makers would suffice to motivate the adoption of efficient and fair policies. The policy maker may privately know the adoption prices; but because this knowledge is not publicly verifiable, any invocation of any adoption price by the policy maker to justify a language policy could be contested by some group. Conversely, if the policy maker is motivated to benefit some group at some other group's expense and thus knowingly bases the language policy on false adoption prices, no victimized group can demonstrate that the true adoption prices justify a different language policy. My model's assumption of policy maker ignorance, then, can best be interpreted as mistrust in the impartiality of the policy maker. Given this mistrust, I have asked whether rules can be designed that will produce efficient and fair language policies, thereby eliminating the need to rely on the discretion of potentially biased policy makers.

So why not design the simplest possible semiobjective language regime? Why not just ask each group what its adoption price is and trust each group to tell the truth? The answer is that under conditions where a more complex language regime works, such a simple language regime does not work. Although I shall not present the proof here, under the same conditions as defined in result 6, if one simply trusts each group to disclose its own adoption price, the groups are not always truthful, and fairness is not always achieved. Specifically, when the efficient language policy would officialize the native language of group 2 and group 1 is the first one to be asked to disclose its adoption price, it overstates its adoption price (without overstating it so much as to make a different set of official languages appear efficient). Group 2 responds to this lie by telling the truth, because it cannot benefit by lying. The efficient language is officialized; but the taxes are based on the falsely inflated adoption price of group 1, so group 1's per capita burden is smaller than group 2's, making the language policy unfair.

Given this defect in a trusting language regime, one could still rely on it if it were possible to know that the first group to disclose its adoption price is the group with the larger adoption price. In practice, one can plausibly guess that this is true of the larger group. The motivation for this guess is that adoption prices are aggregates of individually incurred costs and are thus proportional to group size, except for any systematic intergroup differences. Further, it seems that minority members usually learn majority languages more willingly and easily (through exposure) than vice versa, so intergroup differences would tend to support the guess that the larger group's adoption price is larger. In many countries, language groups differ greatly in size. According to Rustow's (1968) compilation the largest native language is at least twice as large as the next-largest in 80% of all countries. Thus, one can hope to inhibit unfairness by soliciting the larger group's disclosure first.

But it is precisely in countries with similarly sized language groups—or with groups whose languages, despite small size, are plausible candidates for official status—where the cost measurement problem is salient and a self-executing rule might be desired. Thus, elegance and practicality both argue for a redesign of the language regime to preserve fairness against false disclosures, without requiring a guess as to the rank order of adoption prices.

As result 6 has shown, an element of suspicion built into the language regime eliminates the advantage that the first-asked group can gain from lying. In effect, it gives group 2 an opportunity to call group 1's bluff when group 1 would otherwise gain an advantage from lying. Knowing that its bluff can be called, group 1 finds it rational not to lie, so its bluff never needs to get called. The SOPS language regime doesn't eliminate all lying but eliminates lying when lying would make the language policy unfair.

The lying that remains under the SOPS language regime actually protects fairness, rather than undermining it. Under this regime, it is rational for group 1 to lie when an efficient language policy would officialize group 1's native language. Group 1 then understates its adoption price. This lie prevents group 2 from gaining an unfair advantage by overstating group 2's adoption price. Group 1's lie keeps group 2 honest. Since language 1 is the one that becomes official, only group 2's (true) disclosure of its adoption price is used in determining the taxes. Therefore, group 1's lie does not mislead the language regime into producing an unfair language policy. A fairness-irrelevant lie by one group induces the other group to disclose a fairness-relevant truth.

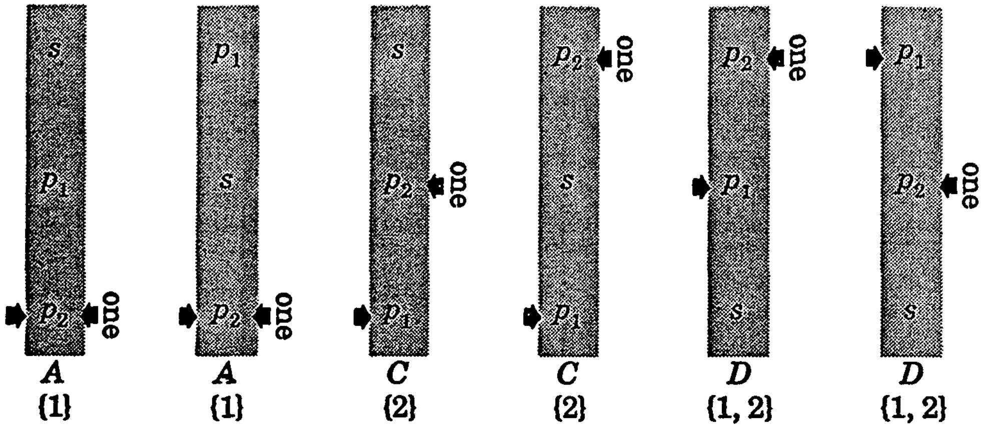
Figure 2 illustrates the groups' responses to the SOPS language regime. Under four out of the six conditions shown, both groups' disclosures are identical to their adoption prices. Under two conditions (in state A), group 1 pretends its adoption price is the same as it knows group 2's to be, so group 2 will have no incentive to pretend its adoption price is larger than it truly is.

Conclusion

My model of a linguistically heterogeneous polity yields reassuring results about the possibility of an efficient and fair language policy. A language policy consisting of a set of official languages and a schedule of taxes on language groups can always be made both efficient (total-cost-minimizing) and fair (individual-burden-equalizing). Furthermore, one can specify a rule that produces a fair (though not efficient) language policy without depending on information about the cost that any choice of official languages would impose on any language group. Finally, if we are willing to let two rational groups give us claims as to the costs they would suffer as a result of official status being denied to their own languages, we can specify a rule that produces an efficient *and* fair language policy. An appropriate revelation procedure can be designed so that any misrepresentation of cost will, rather than preventing an efficient and fair official language policy, actually secure it.

Questions about the robustness of my results are appropriate. One kind of ques-

Figure 2. Signals and Resulting Official Languages in a Two-Language Polity under a Suspicious Own-Price-Soliciting Language Regime



Legend: A = state, {1} = set of official languages, → = group 1 signal, ← = group 2 signals.

Note: p_1, p_2 or s is the true total cost if {2}, {1}, or {1, 2}, respectively, is the set of official languages. The policy maker knows only s. Group i signals a value for p_i. If apparent minimum total costs are tied, the language signaled by group 2 is officialized.

tion is whether the extremity of my assumptions has led to results qualitatively different from those that moderate assumptions would have produced. Most obviously, suppose I had allowed imperfections in the rationality and in the complete information of the language groups. Would result 6 have been fundamentally different? I believe it would have changed only incrementally. Consider state A, as shown in Figure 2. Here, group 1 understates its adoption price, matching the known true adoption price of group 2. Group 2 then truthfully discloses its adoption price. Group 1's language is made official. As a precise empirical prediction, this result is implausible, because of imperfections in information and rationality. If group 1 slightly underestimated group 2's adoption price, group 1 would unknowingly undercut it, and group 2 would then rationally tell the truth and let the language regime make group 2's language the only official language. This could cause group 1 great damage, for its true adoption cost would be p_1 while its compensation would be based on its disclosed price below p_2. What, then, would happen if the model were amended to include reasonable assumptions about uncertainty and miscalculation? Presumably, in state A group 1 would not precisely match its belief about p_2 when it chose a disclosure. Instead, group 1 would allow a margin for error and disclose a price somewhat above p_2, whereupon group 2 would disclose an adoption price between group 1's disclosure and the true p_2. The language regime would still officialize group 1's language. The language policy would still be efficient; and it would be almost fair, only slightly advantaging group 2. Thus, my results appear not unduly sensitive to relaxations in the groups' information and rationality.

Another kind of question is whether I have omitted fundamental aspects of official language politics from this model.

Indeed, I have; and some realistic complications, reflecting known typical features of language politics, deserve inclusion in subsequent models. For example, compensatory differences in tax rates among language groups, used in my model to achieve a fair language policy, would in fact probably lead some native speakers of official languages to attempt to misrepresent their language-group membership. Such misrepresentation has been observed in India, Quebec, and elsewhere; it could be included in a more elaborate model. In addition, official languages, especially when made obligatory media of instruction in schools, can influence intergenerational assimilation. A model that incorporated assimilation would permit one to deal with issues of intergenerational fairness and the value of intergenerational linguistic continuity. Other models could permit defection of groups from the polity or could give citizens a choice whether to learn an official language. The groups could be assumed to have incomplete information about one another's adoption prices. Translation cost could vary depending on the languages. Discounting could be introduced to represent the up-front cost of language learning versus the ongoing cost of translation. New policy norms such as language diversity could be added. Individuals' adoption prices and nonlinguistic policy interests could differ within groups. The official statuses of languages could be permitted to be qualitatively differentiated (e.g., "official," "working") or spatially distributed (e.g., "national," "regional").

While inviting such extensions, I point out that even the present model is complex when compared to those implicit in prior work. The protomodels of official language choice that I have answered here have simply ignored the possibility of nonlinguistic compensation for the inequalities produced by official languages. Theorists addressing the impacts of language policies on population groups have not analyzed the problem of incentives to misrepresent one's costs. Of course, observers of language politics in particular places usually describe trade-offs and deceptions; but we are yet far from making good use of their insights in enriching the general theory of language choice.

Notes

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1. E.g., Andronov 1975; Beer and Jacob 1985; Brass 1974; Coulmas 1985, 113–47; Das Gupta 1970; DeFrancis 1977; Fishman 1986; Haugen 1966; King 1977; Laitin 1977; McRae 1986; Muñiz-Argüelles 1989; Noss 1967; O'Barr and O'Barr 1976; Ostrower 1965; Polomé 1968; Pool 1978; Rabushka and Shepsle 1972; Rubango 1986; Schiffman 1987; Tabory 1980; Tollefson 1986; Weinstein 1990; Whiteley 1974; Wurm 1974; Young 1976, 367–72.

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A "COGNITIVE MISER" THEORY OF COOPERATORS' ADVANTAGE

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We propose a new model of cooperators' advantage depending neither on supplementary incentives nor on cooperators' capacity to recognize, and play selectively with, other cooperators. It depends, rather, on players' making the play–no play decision by the heuristic of projecting their own "cooperate–defect" choices onto potential partners. Cooperators offer to play more often, and fellow cooperators will more often accept their offer. When certain boundary conditions are met, this results in a higher expected payoff for cooperators than for defectors. Empirical support for this heuristic is suggested by expectations data from related social dilemma experiments. Moreover, its use can be justified in Bayesean terms. Our model brings behavioral decision theory's "cognitive miser" paradigm to bear on interdisciplinary concern with the evolution of cooperative behavior and shows how, if other mechanisms provide a suitable "initial kick," cooperation can evolve in the absence of iteration and in large, mobile societies.

> Every man is in his own person the whole human race, with not a detail lacking. I am the whole human race without a detail lacking; I have studied the human race with diligence and strong interest all these years in my own person; in myself I find in big or little proportion every quality and every defect that is findable in the mass of the race. . . . What a coward every man is! and how surely he will find it out if he will just let other people alone and sit down and examine himself.
>
> —Mark Twain

For cooperation to survive and prosper, standard theory requires that it must, somehow, rebound to the comparative advantage of the cooperator. But that is, at best, problematic. In the prisoner's dilemma terms in which the problem is classically formulated, cooperation is a dominated choice; those who cooperate are worse off, by definition, than those who defect. Thus, cooperation appears to be at an evolutionary disadvantage and cooperative tendencies should vanish over time. That outcome is, of course, collectively undesirable because aggregate payoffs are highest in the cooperate–cooperate cell and lowest in the defect–defect one.

Yet cooperation *does* survive, and a major agenda of contemporary social theory has been to explain how it does. Many theorists propose that it happens because the dilemma's incentives are supplemented by others sufficient to make cooperation more rewarding *in net* than defection:

1. Trivers (1971) has shown how kin altruism can promote inclusive or genetic fitness, and Alexander (1979, 1987) has shown how, in the context of indirect reciprocity, cooperation can prove privately advantageous;
2. theories of social evolution (e.g., Boyd and Richerson 1988; Campbell 1975; Richerson and Boyd 1988; Singer

1983), as well as sociological theory more generally (e.g., Parsons 1937), propose that cooperation is maintained by sanctioning systems sufficient to make it, in net, better for the acting individual than noncooperation;

3. Taylor (1976) and Axelrod (1984) have argued—assuming "reciprocal altruism"—that iteration of the game can modify the prisoner's dilemma incentives of single-play games so that contingent cooperation is to each player's private advantage;
4. political theory recognizes that the state (Leviathan) has the capacity to convert the prisoner's dilemma incentives of the state of nature into a nondilemma in which contributing to public goods (in Hobbes's [1947] classic case, social peace) is the rational egoist's dominant choice, so that state power has an ethical mandate based on support from people who understand—explicitly or implicitly—that without it they face multiple prisoner's dilemmas in their interactions with others (see, e.g., Buchanan 1972);
5. elaborating the Hobbesian argument, modern constitutional theory has sought rules for collective decision making that provide private incentives supporting collectively advantageous outcomes ("incentive-compatible designs")—which rules might be deliberately constructed or might evolve as a result of their beneficial consequences for the population (see, e.g., Brennan and Buchanan 1985; Buchanan 1977).

If the individual *must* play the game, the egoistically rational choice is, by definition, to defect. Under that critical constraint, therefore, the *only* way by which cooperation can end up ahead is through a change of the net incentive structure—as proposed by theories such as those just outlined.

A second paradigm is based not on the choice between cooperation and defection per se but on the choice people *can* often make between playing and not playing the game. Peoples' capacity to recognize and play with cooperators and to recognize and *not* play with defectors is pivotal here, and several authors have shown how mechanisms that promote accurate expectations about others' behavior can produce a cooperators' advantage. Frank (1988), for example, has suggested that evolved emotions serve as cue givers about the probable choices of others, thus letting cooperators avoid interaction with defectors and reap the benefits of mutual cooperation. In the same vein, Gauthier (1986) has discussed *translucency*, a characteristic of behavior that provides a basis for including or excluding individuals from cooperative play. Note that this second, selective-play paradigm depends on the existence of a "cooperative surplus" (Gauthier 1986), that is, on a larger net payoff being available at mutual cooperation than at any other outcome. In terms of this paradigm, however, cooperation remains strictly a dominated choice.

We propose a new model that is cast in the broad terms of the selective-play paradigm, but in which the cooperators' advantage does not depend on cooperators' capacity to recognize other cooperators or to unmask would-be defectors. In our model, cooperators win but not because they and their targets are, to some degree, translucent. In fact, while our model requires both cooperators and defectors to *believe* they can spot cooperators, it builds from the conservative assumption that their performance is no better than random with respect to the facts of target behavior. That is, cooperators' and defectors' judgments about whether someone else is a cooperator or defector are *noncontingent.* Our selective-play model of cooperators' advantage is, therefore, distinct from Frank's (1988) and Gauthier's (1986) versions, both of which depend on expectations about successive individuals having some predictive accuracy.

Instead of translucency, our model assumes that (1) judges project their *own* choices between cooperation and defection onto the choices of targets, that is, judges' expectations are developed from the heuristic that their own choices are diagnostic (Dawes 1988a) of others' behavior; and (2) judges (cooperators and defectors alike) make their individual-by-individual play-no play decisions on that basis. In these terms, as we will show, it is possible for cooperators to have higher expectations across a set of *options* to play than do defectors despite the higher per-play payoff that, by definition, adheres to defection.

The model has several attractive features.

1. It identifies circumstances under which cooperators will end up ahead of defectors without the awkwardness of resolving the dilemma by eliminating it and without the information demands of models that depend on translucency. As we will show, the process we identify is quite compatible with such more standard processes; indeed, it can build on whatever base of cooperation those processes establish—even needs them to operate before it can operate. It is, however, in many ways a much *simpler* process.
2. It brings behavioral decision theory's "cognitive miser" (Tetlock 1979) paradigm to bear on sociobiology's concern with the survival of cooperation. During the past 20-or-so years, experimental studies have challenged subjective expected utility as a descriptive model of human behavior, identifying a set of heuristics, or rules of thumb, that can be explained (and perhaps justified) insofar as they economize on cognitive effort (for surveys, see Bell, Raiffa, and Tversky 1988; Dawes 1988a; Hogarth and Reder 1987). Until now, however, this work has been independent of the no-less-formidable work in human sociobiology, whose major problem, according to Edward Wilson (1975), is to explain cooperative behavior.
3. It demonstrates how factually wrong belief in one's own judgment can help as well as hurt, consistent with David Wilson's (1980) general insight that an unrealistic mental model may have adaptive features.
4. It shows that cooperation *can* survive and flourish even among strangers interacting in large mobile societies. The model's implications are distinct, therefore, from those of other models that find support for cooperation in small face-to-face groups and that base cooperation in larger societies on extrapolation from there (e.g., Masters 1989; see also Buchanan 1965, 1978).

The Model

Parameters

Assume a population of n people who have the capacity to play two-person prisoner's dilemma games with each other, the games having payoffs $t > c > 0 > d > s$ (with the standard meanings).[1]

ASSUMPTION 1. *Each person makes a single decision whether to cooperate or defect across the set of such games—if they play them. Cooperators always cooperate and defectors always defect.*

People might cooperate because of normative or ethical considerations or because of selective incentives provided by Leviathan or some other institution or in response to internalized altruistic motivations. And they might defect because such mechanisms are insufficient to persuade them otherwise. But reasons for their cooperate–defect decisions are exogenous to the model. We assume simply *some* incidence of cooperation and of defection.

The assumption that people make a single cooperate–defect choice good for all consummated plays is for analytic purposes only and need not imply two fixed "types." In fact, the model is quite compatible with individuals' choices alternating between cooperation and defection as a consequence of whatever processes are promoting those behaviors at the time and, therefore, with the aggregate incidence of such choices also fluctuating from time to time. The model only requires that at the *point of choice* people are either cooperators or defectors.

ASSUMPTION 2. *Each person decides whether or not to play one such game with each of the others, and each player has a veto on play with each other player. A game is consummated and play completed only when two players agree to play with each other.*

The model implies no particular *sequencing* of offers to play and acceptances or rejections of those offers, such as would invite game-theoretic analyses. While it is possible that players in natural situations do sometimes adapt their cooperate–defect choices to others' invitations to play, for example, we are interested simply in the consequences of their making play–no play decisions by the heuristic in question, and cooperate–defect choices are a given in our model. For simplicity, therefore, imagine all players as making their offers to play (or refraining from such offers) at the same time.

Since t and $c > 0$ and d and $s < 0$, people will play if and only if they expect that the person they have met will cooperate. Cooperators wish to play with other cooperators because that is the only way they can make more than 0, while defectors wish to play with cooperators for the same reason. Of course, $t > c$ means that defectors make a higher return than cooperators from each successfully consummated play with a cooperator.

We model expectation formation in the particular case as subjects' taking a random sample of one from a probability distribution defining the expected incidence of cooperation in the population. Players assign some probability to meeting a cooperator and, in effect, bet in those terms that a newly met person is a cooperator. Bias one way or another in a player's expectations simply moves the probability of play and no-play decisions in the direction of the bias. It does not determine that choice: players might play even if their expectations are biased away from cooperation, and might decline to play even if their expectations are biased toward cooperation. A player's probability of being correct is, of course, defined by the actual incidence of cooperators and defectors in the population.

In natural situations, no doubt, people choose their partners by a number of criteria; but we are interested in the consequences of one that is among the simplest —projecting their own behavioral intentions onto others.

ASSUMPTION 3. *Whatever their cooperate–defect choice, people are biased toward expecting others to do the same as themselves. Cooperators judge the proportion of cooperators to be q while defectors judge it to be r, where $q >> r$.*

People are assumed to be biased in this manner whether their cooperate–defect choice is based on exclusive attention to private prisoner's dilemma incentives, on supplementary incentives (e.g., from Leviathan or decentralized normative systems), on religious or ethical principles, on the anticipation of long-term gains through reputation effects, on a "perception of themselves as individuals strongly linked to others through a shared humanity" (Monroe, Barton, and Klingemann 1989), or anything else. Nor does it matter whether the projection involves generalizing from the specifics of their own circumstances and motivations or, more simply, generalizing just from their own choice per se. All the model requires is that people figure out other people, in Mark Twain's phrase, by examining themselves.

Empirical Justification

Social Dilemma Data. In a succession of experimental studies exploring the circumstances surrounding cooperation in n-person prisoner's dilemmas, we have collected data about subjects' expectations of others' behavior.[2] One of our most consistent findings throughout these studies —a finding replicated by others' work—is that cooperators expect significantly more cooperation than do defectors. This result has been found both when payoffs are "step-level" (when contributions from a subset of *k* subjects ensure provision of a benefit to all) and when they are "symmetric" (when all contributions ensure a constant benefit to all). Table 1 reports data from a study with the former design, and Table 2 reports data from one with the latter design.

In the linear case (Table 2), we report expectations by discussion–no discussion conditions and by the target's actual cooperate–defect choice, as well as by the judge's choice. Note that cooperation is more frequent in discussion conditions than in no-discussion conditions ($p <$ .001); discussion works in *some* way to increase the base of cooperation.[3]

Linkage between Behavior and Expectations. While game-theoretic formulations treat expectations about others' behavior as conditions of the actor's own behavior, an early study by Dawes, McTavish, and Shaklee (1977) reported findings consistent with expectations' being dependent on the actor's own behavior—as required by the present model. These authors solicited expectations not only from sub-

Table 1. Mean Expectations of Others' Cooperation by Judges' Cooperation–Defection Choices in a Step-Level Dilemma Experiment

	Probability of Being		
Dilemma Type	Futile	Critical	Redundant
Standard			
Cooperators	.24	.29	.46
Defectors	.55	.18	.26
Money-back guarantee			
Cooperators	.25	.29	.46
Defectors	.28	.21	.50
Enforced contribution			
Cooperators	.20	.23	.57
Defectors	.51	.25	.24

Source: Dawes et al. 1986, 1181.

Note: Seven-person groups were presented with a step-level dilemma in which a "bonus" of $10 for each member was provided for all members in the event that four or more contributed a $5 endowment toward that good. In the *standard dilemma,* contributing when three or less contributed made the contribution *futile* (a subject would lose his or her endowment); contributing when three others also contributed made the contribution *critical*; and contributing when four or more others contributed made the contribution *redundant.* In the *money-back guarantee* dilemma, subjects would get their contribution back if enough others did not contribute to produce the good. In the *enforced contribution* dilemma, subjects would be obliged to contribute if contributions from others were sufficient to produce the good. Expectations were measured by the method outlined in note 2. Relevant to this argument, there is a main effect for cooperation ($F_{1,102} = 26.36$, $p < .001$).

Table 2. Mean Expectations of Others' Cooperation by Judges' and Targets' Own Choices and by Discussion–No Discussion in a "Linear" Social Dilemma Experiment

	Condition and Judges' Choice			
	Discussion		No Discussion	
Target Choice	Cooperate	Defect	Cooperate	Defect
Cooperate	.81	.56	.63	.36
Number of cases	(104)	(104)	(68)	(74)
Defect	.57	.32	.61	.34
Number of cases	(83)	(98)	(123)	(148)

Note: The experiment on which these data are based is reported more fully as experiment 1 in Orbell, van de Kragt and Dawes 1988. This table reports expectations derived by the first method outlined in note 2. For each subject (the target) we averaged expectations held by others in the group who cooperated and defected (the judges); where one or more of the up-to-six judges in each behavioral category failed to report expectations (or misunderstood how to record their expectations), we deleted the case. Subjects had to choose between "giving" a six-dollar endowment to six "others in the experiment" (in which case each of the six benefited by $2) and "keeping" that endowment for themselves. The original design crossed (1) initial understandings about *who* would receive the benefit of a decision to give (the six others in one's own group or six matched individuals in a second group), (2) group discussion (10 minutes among the seven people in each group or no such discussion), and (3) subsequent understandings about who would receive the benefit of a decision to give. Cooperation rates varied substantially across the resultant eight conditions, but for present purposes we report expectations only by discussion.

jects who actually chose between cooperation and defection in a social dilemma game but also from subjects who only observed the game through a one-way mirror and did not make actual choices themselves. The authors reasoned that if expectations are developed independent of the individual's own choice, the standard deviation of expectations in the two cases should be similar. If, on the other hand, expectations are to some extent derived from subjects' own choices, the standard deviation of expectations for choice makers should be greater than the standard deviation for those who simply observed, because people who choose to cooperate will be led to expect cooperation, and people who choose to defect will be led to expect defection. They found that the figure for choice makers was, in fact, greater than for those who simply observed.

As Dawes (1980) has pointed out, in n-person dilemmas as distinct from two-person ones, (1) harm for defection is spread out over many players; (2) behavior is likely to be anonymous; and (3) except when all others simultaneously choose cooperation or defection, players have little or no reinforcement control over specific other individuals. No doubt these differences mean that empirical findings from social dilemma experiments should be extrapolated to two-person prisoner's dilemma games only with caution. We believe, however, that the formal similarities between the two paradigms (both involve a dominant incentive associated with a suboptimal social equilibrium) make a similar relationship between expectations and behavior in the two cases likely.

Theoretical Justification

In developing expectations about others' behavior, an individual does have information about his or her own actual or intended choice; and *in the absence of information about any other individuals' likely choices*, a reasonable assumption (at least for a Bayesean) is that his or her own choice is representative of how others will choose. Of course, if information about others were available, it would be appropriate to incorporate it into a revised estimate. But without such additional information, extrapolating from one's own choice is a reasonable thing to do. We can predict, therefore, that when the problem involves sets of strangers interacting for the first time, subjects' expectations of others' choices will be substantially biased by their own choices. (See Dawes 1988b for development of this theme in the context of the literature on "false consensus"—which literature, of course, provides further empirical support for a general tendency of people to expect others to be like themselves.)

Expected Payoffs

We can now derive expected payoffs for cooperators and defectors. Since the relative expectations are unaffected by scale changes, let $d = -1$ and, to avoid confusion, $s = -z$. A cooperator proposes play to another person with a probability q. Then, that person is

1. a cooperator with probability p, in which case the person agrees with probability q, and the payoff to the proposer is c; and
2. a defector with probability $1 - p$, in which case the person agrees with probability r, and the payoff to the proposer is $-z$.

Note that both cooperators and defectors will accept a proposal to play only if they believe the proposer is a cooperator; those who are cooperators themselves accept with an expectation of receiving c (> 0) from mutual cooperation, while those who are defectors accept with an expectation of receiving t (> 0) from unilateral defection. Hence, the expected payoff to a cooperator on encountering some other individual is $q(pqc - (1 - p)rz)$.

In contrast, a defector who meets another person proposes play with probability r. Then, that person is

1. a cooperator with probability p, in which case that person agrees to play with probability q, and the payoff to the proposer is t; and
2. a defector with probability $(1 - p)$, in which case that person agrees with probability r, and the payoff to the proposer is -1.

Hence, the expected payoff to the defector on encountering some other individual is $r(pqt - (1 - p)r)$. The difference is

$$pq(qc - rt) - (1 - p)r(qz - r), \qquad (1)$$

which can be positive. We term expression 1 the *cooperators' advantage* (which can, of course, be negative).

For example, let $t = 2$, $c = 1$, $z = 2$ (i.e., $s = -2$), $d = -1$, $p = .5$, $q = .8$, and $r = .2$. I, a cooperator, offer play with probability .8. The other individual is a cooperator *and* accepts with probability $.5 \times .8 = .4$, in which case my payoff is 1; or the other individual is a defector *and* accepts with probability $.5 \times .2 = .1$, in which case my payoff is -2. Hence, upon encountering someone, my expectation is $.8\ (.4 \times 1 - .2) = .16$. On the other hand, you, a defector, offer play with probability .2. The other individual is a cooperator *and* accepts with probability .4 (again $.5 \times .8$) and your payoff is 2; or the other is a defector *and* accepts with probability .1 ($= .5 \times .2$), in which case your payoff is -1. Hence, upon encountering someone, your expectation is $.2\ (.4 \times 2 - .1) = .14$. I have a higher expectation.[4]

Boundary Conditions

This model is restrictive. It can only produce a cooperators' advantage when d (therefore z) is negative and defectors lose some value from interacting with other defectors; clearly, natural prisoner's dilemmas in which all values are positive do exist.

Further, the model requires setting t, c, d, z, p, and r and then solving for q; and solutions *might* not be in the (0, 1) interval. Critical parameters are

1. $p/(1 - p)$, the ratio of cooperators to defectors in the population—the higher this ratio, the more opportunities cooperators have for playing with other cooperators and for capturing, therefore, cooperators' surpluses;
2. q/r, the ratio of cooperators' to defectors' optimism about cooperation in the population—the higher this ratio,
    a. the greater cooperators' gains from playing with other cooperators (pqc) relative to defectors' gains from playing with cooperators (pqt) and
    b. the smaller cooperators' losses from playing with defectors ($(1 - p)rz$) relative to defectors' losses from playing with other defectors ($(1 - p)r$); and
3. z and t—the possibility of a cooperators' advantage when cooperators and defectors play together[5] is a negative function of z and t summed.

To define the possibility of a cooperators' advantage in these terms, normalize expression 2 relative to defectors; specifically, divide by $(1 - p)r^2$:

$$\frac{p}{(1-p)}\left(\frac{q}{r}\right)\left[\left(\frac{q}{r}\right)c - t\right] - \frac{(1-p)}{(1-p)}\left[\left(\frac{q}{r}\right)z - 1\right] . \qquad (2)$$

Let $P = p/(1 - p)$, the ratio of cooperators to defectors; let $\alpha = q/r$, the ratio of cooperators' to defectors' expectation of cooperation. The cooperators' advantage is

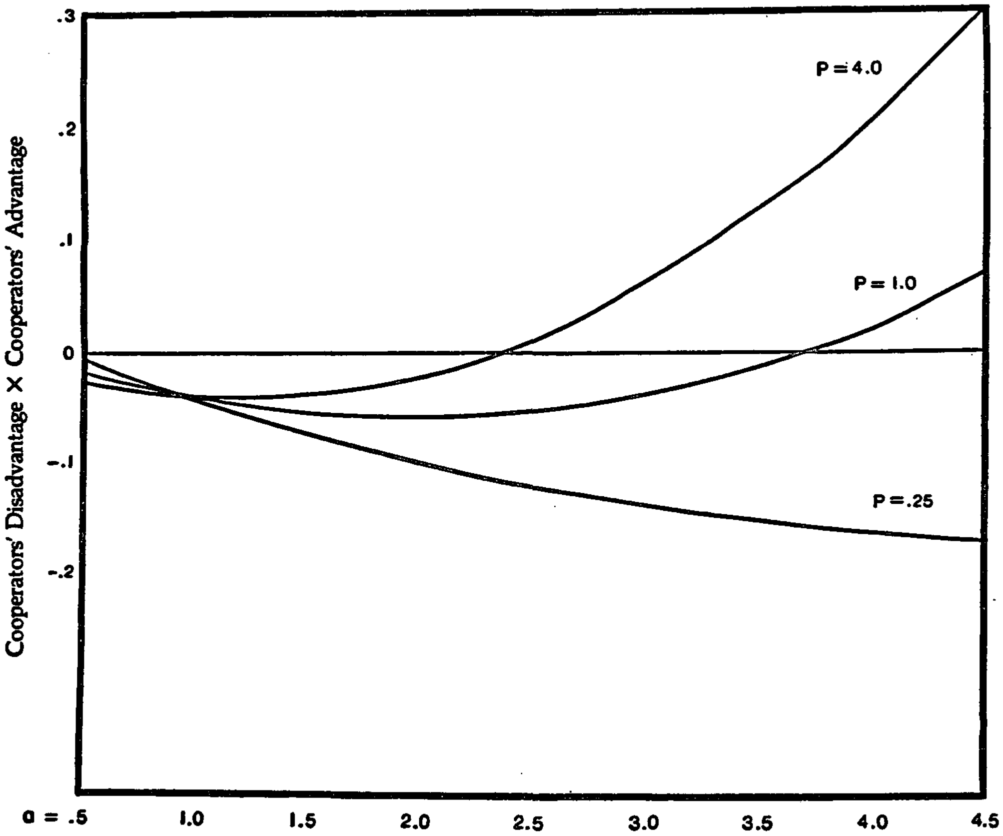
$$(2) = P\alpha\,[\alpha c - t] - [\alpha z - 1]$$
$$Pc\alpha^2 - \alpha(Pt + z) + 1. \qquad (3)$$

Note that expression 3 is a linear function of P, c, t, and z. It is always a positive linear function of c and a negative one of t and z, which is consistent with the meaning of these payoffs. In contrast, it is a positive linear function of P if $c\alpha - t$ is positive, otherwise negative. (Note that by assumption $\alpha > 1$.) To define the quadratic role of α, consider the derivative of expression 3 with respect to α, which equals $2Pc\alpha - (Pt + z)$. Thus, the sum of Pt and z reduces the value of the slope in a linear way; if expression 3 is positive, increasing their sum makes a cooperators' advantage less likely, and decreasing their sum makes it more likely.

In contrast, α and P are multiplicative. Assuming that c, t, and z are set, we have an "explosion" effect: the more cooperators there are in the population, the more beneficial to them is their relative optimism, which in turn is more beneficial the more cooperators there are.

For example, Figure 1 sets t, c, d, and z as in the previous case and shows how expression 2 varies as a function of increasing α values for three values of P. The middle P value, where the population is evenly divided between cooperators and defectors, is the one from which the previous example was drawn; as can be seen, cooperators' expected payoff moves above the break-even point when α is between 3.5 and 4.0. On the other hand, with P at 4.0, cooperators have a clear advantage before α reaches 2.5. Note that the upper two functions, at least, are *U*-shaped; up to a point, cooperators are advantaged by *less* optimism about others; but beyond that point increasing

Figure 1. Cooperators' Advantage



Matrix:	1, 1	-2, 2
	2, -2	-1, -1

α = Ratio of cooperators' expectations to defectors' expectations.
P = Relative proportion cooperating in the population.

optimism pays—and pays increasingly well. Also note that all three functions would be somewhat higher if the difference between t and z were smaller. The interaction, however, is clear.

Adjustment

Colleagues to whom we have presented our model have suggested that as the size of the available sample expands through a succession of experiences, people will place less and less reliance on their own choice as a basis for predicting others' choices—that the sample will become progressively representative and that therefore beliefs of cooperators and defectors about the proportion of cooperators in the population will converge to the actual proportion. Under those circumstances, any advantage that cooperators might have had from the heuristic would evaporate.

There are reasons for expecting much less adjustment than this argument implies. First, the *need* for adjustment might not seem particularly pressing. While cooperators are involved in a higher proportion of consummated plays than defectors,[6] both experience the same high proportion of cooperative responses $= pq/(pq + (1 + (1 - p)r))$, a proportion, note, that must always be higher than p, the "veridical" proportion of cooperators.[7] Our model requires both cooperators and defectors to *believe* that they can spot cooperators; and experience in consummated plays will simply confirm their ability to discriminate.

Second, the model itself predicts differential expectations even if the same decision is repeated with outcome feedback. While cooperators and defectors both experience more cooperation than defection, both also have a high rate of not playing—either because they do not offer to play or because an offer is rejected. Moreover, the defectors have a much higher rate of not making offers, with relative proportions equal to $(1 - r)/(1 - q)$. Hence, assumptions consistent with prior belief about the propensities of the people with whom games are not consummated can leave the belief about the overall base rate of cooperation in the population minimally affected—or even unaltered—by the feedback from the consummated plays.

In the example we used above, a defector would, like a cooperator, observe an 80% cooperation rate among other players in the games actually played. But a defector offers to play with a probability of .20 and the offer is accepted with a probability of .50 $(.50 \times .80 + .50 \times .2)$, leaving the defector with actual playing experience among only 10% of the population. If the defector were to believe that only .13 of this remaining 90% would have cooperated in consummated play (a figure not *very* discrepant from .20), the base rate estimate of .20 would remain unchanged.[8]

Third, the experimental evidence we cite provides no compelling reason to expect much adjustment.[9] As we have emphasized, our model refers to a single decision taken at a particular point in time. While the decisions in the experiments we cite were made in the laboratory, they were nevertheless just such decisions. The people making them came to the laboratory, no doubt, with lifelong experience in situations with prisoner's dilemma structure; but the evidence that cooperators' expectations of cooperation far exceed defectors' in these single decisions is, as we have shown, nevertheless very strong.

Group discussion in the linear dilemma case presented above (Table 2) does provide subjects with information about others' intentions (however discounted); and information is central to the issue of adjustment. In the absence of discussion, there is no contingency between targets' actual choice and judges' expectations—regardless of whether the judges were themselves cooperators or defectors. With discussion, however, the contingency is clear in both cases. More relevant to adjustment, the difference between cooperators' and defectors' expectations is present in both discussion and no-discussion conditions.

Information about others' intentions gathered from group discussion is not, of course, the same as information gathered from experience with successive plays; but it is information, nevertheless. While that information does increase subjects' accuracy, it does not reduce the *difference* between cooperators' and defectors' expectations—does not reduce, that is, the propensity of cooperators and defectors to expect others to behave as they themselves behave.[10]

It is easy to spin different hypotheses. One is that cooperators adjust more readily than defectors. Defectors encountering cooperators could well explain the event in terms of their own capacity to "spot suckers" and, accordingly, refrain from adjusting; cooperators encountering defectors, on the other hand, will have no such rationalization available to them. Early work by Kelly and Stahelski (1970) is consistent with this speculation, showing that cooperators encountering defectors are more likely to change their behavior (although not their cooperative disposition) than are defectors encountering cooperators.

Alternatively, cooperators might adjust *less* readily: Their larger number of consummated plays comprises a more reliable base for predicting to the whole population and, therefore, less reason for doubting the information it provides.

Another hypothesis says that the learning cooperators take from successive experiences is more polarized than the learning that defectors take. Cooperators encountering other cooperators will be reinforced for their generalized expectation *and* for their capacity to predict in the particular case; encountering defectors, they will be reinforced for neither. On the other hand, defectors encountering cooperators will be reinforced for their capacity to predict in the particular case but not for their generalized expectation; encountering other defectors, they will be reinforced for their generalized expectation but not for their capacity to predict.

The issue is ultimately empirical, of course. Yet adjustment becomes important only when we wish to predict behavior in a particular, ongoing social or interactive situation. Our immediate concern is to show only that the expectations heuristic is sufficient—under appropriate circumstances—to produce a cooperators' advantage and to provide a basis for evolutionary expansion of that advantage. The extent to which it is used in natural situations and the circumstances under which it is rejected are matters for further development.

Discussion

The expectations heuristic is not sufficient to produce a cooperators' advantage in the absence of other mechanisms supporting it. If there are no other cooperators in the population, to illustrate with the extreme case, a single cooperator will lose absolutely and comparatively from every play he or she consummates.[11]

Yet this is not a particular weakness of the model. Despite some extravagant claims (e.g., for the necessity of "incentive compatibililty"), it is unlikely that so basic a process as cooperation is explained by a single mechanism; and interactions (e.g., with Hobbesian coercion or evolved norms) are to be expected. Maruyama (1963) has shown that an "initial kick" (which might never be known) can trigger a deviation-amplifying mechanism that propagates and enhances that initial change. The heuristic we have discussed could act as just such a deviation amplifier in the case of cooperative behavior.

We have assumed that all individuals can interact with all others in the population; but relaxing that to allow a greater probability of interacting among adjacent pairs could give cooperators a higher probability of interacting with other cooperators in certain locations even if there were relatively few in the wider set. In evolutionary terms, the cooperators' surplus thereby produced would increase the relative strength of cooperation in such locations in the next generation, with strength feeding on strength through successive generations.[12]

While we assume that expectations (of cooperators and defectors alike) have only random predictive capacity, we do not deny that translucency might have evolved. As Gauthier (1986) suggests, there is a continuum between transparency, translucency, and opaqueness. Transparency, of course, would ensure a cooperators' advantage; if everyone could recognize everyone else's intentions, no defector would enjoy a consummated relationship. If arguments such as Frank's (1988) on the role of emotions in cue giving, and Cosmides' (1989) on special purpose cognitive mechanisms for detecting cheating are correct, people might be to *some* extent translucent—certainly more than is implied by our conservative assumption of a random hit rate.

It might be suggested that our heuristic identifies the lower bound of cooperators' advantage that is available from selective play. Perhaps, but in the absence of group discussion, we have little evidence of the capacity to predict others' behavior, as required by other models of cooperators' advantage from selective play. In Table 2 we have shown that while judges were reasonably successful in discriminating between cooperating and defecting targets after group discussion, they were not at all successful without it. Similarly, Dawes, McTavish, and Shaklee (1977) found significant accuracy in communication conditions (which, they comment, is "not exactly surprising") but otherwise found little more than random accuracy in one condition, and a slight effect in the opposite direction in another.[13] Further empirical investigation might show that the heuristic discussed here is the *only* basis for cooperators' advantage via selective play when there is no opportunity for sustained, personalized interaction.

Our model incorporates the everyday observation that people who are themselves untrustworthy in joint ventures tend not to trust others and are less willing than optimistic cooperators to seek out or agree to such ventures. Successful enterprise requires that offers for cooperative interaction be accepted by others who themselves have a stake in that interaction; and our model shows how entrepreneurs who intend honest cooperation can, in fact, end up ahead of those who intend exploitation of potential partners. While the latter *are* exploited less often than the former, they also miss opportunities for personal gain that depend on mutual cooperation.

Perhaps paradoxically, the cooperators' advantage we have identified is not available through calculated, strategic play.[14] It depends, simply, on optimism about others' behavior; and such optimism cannot be invented in the interest of promoting private advantage.

Notes

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1. That is to say, t = *temptation* resulting from unilateral defection; c = *cooperate* resulting from mutual cooperation; d = *defect* resulting from mutual defection; and s = *sucker* resulting from unilateral cooperation. Of course, the values specified here represent only a subset of possible prisoner's dilemma games. In some cases, all values will be

positive (with players making gains of varying magnitudes by comparison with the status quo); and in some cases, all values will be negative (with players making losses of varying magnitudes).

2. Although the specifics have varied somewhat between experiments, we have two basic techniques for eliciting such expectations. First, we ask each subject to assign a score between 100 and 0 to every other subject, the particular value indicating "how probable" the judge sees the target's cooperation (X-choice); a score of 50, of course, indicates that the judge cannot decide. Second, we ask each subject to assign a score between 100 and 0 to each of the possible outcomes at the group level (e.g., 0 choose X; 1 chooses $X \ldots , n$ choose X) with the magnitude of the score indicating "how probable" the judge believes that outcome to be and the scores for all $n + 1$ outcomes summing to 100.

3. Orbell, van de Kragt, and Dawes (1988) identify discussion-induced group solidarity and promise making under a rule requiring everyone in the group to have promised as possible explanations of discussion's much replicated effect; but it remains to separate the roles of these two variables or to identify possible interaction between them.

4. Transaction costs might modify this outcome. The cooperators' advantage depends on cooperators' having more plays (with cooperators) than do defectors, and it could be reduced by per-play costs of interacting. Alternatively, the issue of transaction costs could be dealt with by subtracting them from the payoffs in the matrix. However, transaction costs seem likely only to reduce the possibility of a cooperators' advantage, not eliminate it.

5. An error, notice, for cooperators but a success for defectors.

6. Because of this, the set of consummated interactions will be a poor base from which to assess the incidence of cooperators and defectors in the general population. Cooperators' optimism about others means that cooperators will be overrepresented in that set, just as defectors' pessimism means that defectors will be underrepresented. Observing that, say, 75% of players in consummated interactions cooperate, an optimist might conclude that 75% of the population is cooperative. The true figure would be substantially lower.

7. Note, in addition, that if it is also higher than q (hence r), both cooperators and defectors are reinforced in their belief that they are good at spotting cooperators (i.e., are "above average judges of character"—like everybody else).

8. Of course, the implications that cooperators and defectors draw from others' rejecting their offers of play remains an empirical question. Given everyday observation and empirical data about "projections" from trustworthy and untrustworthy people, it seems possible that defectors believe they are turned down because cooperators "see through" their defecting intentions, while cooperators interpret such rejection as reflecting a neurotic lack of trust in their own cooperative intentions.

9. The extensive literature on iterated two-person prisoner's dilemmas is relevant to adjustment when subjects play a series with the same individual; but that is an issue distinct from adjustment when subjects play single games with a succession of different individuals from some population. Hoch and Loewenstein (1989) have provided evidence that at least one other decision heuristic, "hindsight bias," is resistant to outcome feedback. Such feedback can reduce the effect of that bias, but not always; in some cases, the heuristic is strong enough to overwhelm the information contained in the feedback (see also Sherman, Presson, and Chassin 1984).

10. This despite the fact that discussion substantially increases the incidence of cooperation—meaning that (in Figure 1's terms) α does not vary with P.

11. Generally, with prisoner's dilemma payoff parameters constant, a cooperators' advantage requires that the difference between q and r become greater as the proportion of cooperators in the population becomes smaller. Empirical investigation might reveal a threshold of cooperation that must be reached before the mechanism we have identified can begin to operate to cooperators' advantage.

12. Comparable arguments are made by Axelrod (1984) and by Vanberg and Buchanan (1988).

13. Frank (1988) designed experiments explicitly to assess subjects' capacity to predict cooperation after a thirty-minute "get acquainted" period during which they were free to discuss any subject they wished. Consistent with the data in Table 2 and with Dawes, McTavish, and Shaklee (1977), he found considerable predictive success. He did not, however, test such predictive accuracy in the absence of discussion.

14. As pointed out to us by Thomas Palfrey (personal communication 23 May 1990), strategic behavior that allows choice of the highest expected value strategy (i.e., defection) after assessing others' cooperative propensity on the basis of one's own effectively erases the cooperators' advantage.

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RESEARCH NOTES

MONOTONICITY IN ELECTORAL SYSTEMS

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Much of the literature concerning the relative merits of alternative electoral rules is centered around the extent to which particular rules select "representative" legislatures. And an important concern in evaluating the "representativeness" of an electoral rule is whether or not the rule responds positively to changes in individuals' preferences, that is, whether or not the rule is monotonic. By explicitly considering electoral rules in the context of a complete electoral system—voting, selection of legislature, and legislative choice of policy—we argue that monotonicity in electoral systems is a nonissue: depending on the behavioral model governing individual decision making, either everything is monotonic or nothing is monotonic.

Broadly defined, a representative electoral system is a decentralized method for translating the preferences or interests of an electorate into policies via the selection of agents to a legislature. We can think of this process as the culmination of three distinct stages: (1) individuals make voting decisions based in some manner on their preferences; (2) the electoral rule in place determines who is elected to the legislature; and (3) the members of the legislature interact to derive a policy outcome. While outcomes from the second stage are determined mechanically, those from the first and third stages depend inter alia on strategic decisions of voters, candidates, and elected officials.

We have previously developed two alternative models of the legislative stage 3 and, in one case, examined the implications of the model for behavior at stage 1 (Austen-Smith and Banks 1990, 1988). Now, however, we suppress issues concerning behavior at the legislative stage and instead focus on stages 1 and 2, that is, on methods for translating votes into legislatures and the associated behavior of the voters.

Much of the literature concerning the relative merits of alternative electoral rules is centered around the extent to which particular systems select "representative" legislatures. And an important concern in evaluating the representativeness of an electoral rule is whether or not the rule is monotonic. *Monotonicity* is a criterion capturing the idea that social choices should reflect positively changes in individuals' preferences: "It would be perverse in the extreme if increased votes for an alternative contributed to its defeat. Consequently, it seems an elementary requirement of sensible and fair choice that the decision rule respond positively, or at least non-negatively, to increase in individual evaluation of an alternative" (Riker 1982, 45). It is often argued, however, that many rules—in particular, "elimination" rules such as single transferable vote (STV) or runoff—violate monotonicity and thus are essentially undesirable (e.g., Brams and

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Fishburn 1984; Doron and Kronick 1977; Fishburn 1982; Riker 1982).

However, we argue that such a conclusion is unwarranted for two reasons. First, it is not the relationship between votes and legislatures that is of fundamental interest but rather the relationship between the underlying voter *preferences* and legislatures. The degree to which an electoral system is representative should not be judged with respect to voting decisions (i.e., processes) but rather with respect to actual preferences over outcomes (i.e., consequences). Analyzing this relationship between preferences and outcomes evidently requires a model of how voting behavior itself responds to changes in voters' preferences. Rather than specifying any particular model, however, we proceed by simply assuming the existence of some behavioral model of voting. We then show that if this model generates a unique prediction of how individuals vote for all possible profiles of underlying voter preferences, then for virtually all electoral rules the translation of voter preferences into legislatures via the behavioral model and the electoral rule will be nonmonotonic. For example, since "sincere" voting under most electoral rules gives a unique behavioral prediction, the resulting process will generate a nonmonotonic relationship between voter preferences and legislatures. On the other hand, the converse result holds if one admits behavioral predictions that are not necessarily unique for all profiles of underlying preferences. In particular, if one assumes the voters adopt Nash equilibrium strategies, then for *any* electoral rule the resulting translation of preferences into legislatures will be monotonic.

The second reason is that even if one has an intrinsic interest in process-oriented monotonicity, it turns out that such a requirement is not satisfied by *any* reasonable electoral rule. As remarked earlier, the literature contains several examples to illustrate the process-oriented nonmonotonicity of particular electoral rules such as STV and runoff. This result implies that such examples can be constructed for virtually *all* electoral rules. Thus, our main conclusion is that monotonicity/nonmonotonicity in electoral systems is a nonissue.

Finally, it is worth emphasizing that although to our knowledge our arguments are novel, they are predicated on known results in the social choice literature concerning the existence of strategy-proof voting mechanisms.[1]

The Model

Consider a political environment consisting of a set $N = \{1, \ldots, n\}$ voters, in a single district (the extension to multiple districts is straightforward). In a legislative election, the voters elect ℓ representatives to the legislature. Let K denote the set of candidates competing for the district's seats; candidates may or may not be affiliated through some party structure. Thus $L = \{\lambda \subseteq K : |\lambda| = \ell\}$ is the set of all possible legislatures selected by the voters.

Voters determine a legislature through an *electoral mechanism* (V, γ), where $V = \mathop{\times}\limits_{N} V_i$ and V_i is voter i's strategy set, and γ is a mapping $\gamma : V \rightarrow L$; we refer to γ as the *electoral rule.* Thus, given an electoral mechanism (V, γ) and a strategy profile $\mathbf{v} \in V$, the resultant legislature is given by $\gamma(\mathbf{v}) \in L$. Here we consider only the electoral rules for which the strategy sets of individuals $\{V_i\}$ can be equated with the set of orderings R_i. Thus for all i, $V_i = \mathbf{Q}(L)$, the set of orderings of L.[2]

Once the legislature L has been determined, elected representatives select a policy outcome from some arbitrary feasible outcome space, X. Since our principal focus here is on the effects of various electoral laws, we suppress the behavioral model of the process by which legislatures select outcomes by positing a reduced form expression $\sigma : L \rightarrow X$ summarizing the outcome of legislative decision making. Call σ a *legislative outcome function.* So given a strategy profile **v**, voters determine policy outcomes in X through $\sigma(\gamma(\mathbf{v}))$.

So far we have described the second and third stages of an electoral system. To complete the picture, it remains to describe how individuals select their voting strategies. Rather than restrict attention at the outset to one behavioral model, we define an abstract behavioral rule as a mapping that assigns to every possible profile of voter preferences a profile of decisions. Formally, let β: $\mathrm{Q}(L)^n \twoheadrightarrow \mathrm{Q}(L)^n$ denote an arbitrary behavioral correspondence, where β may be functionally dependent on the electoral rule γ (e.g., the Nash equilibrium correspondence defined later).

Finally, we assume voter $i \in N$ has well-defined basic preferences $\geq_i$ on X. Given these basic preferences and given the legislative outcome function σ, i has *induced* preferences R_i over the set of legislatures L given by, $\forall \lambda, \lambda' \in L$: $\lambda R_i \lambda'$ if and only if $\sigma(\lambda) \geq_i \sigma(\lambda')$. By definition, the structure of the domain of induced preferences over L depends on the structure of the domain of preferences over policy outcomes, X, and on the details of the legislative outcome function, σ. In particular, unrestricted domain on X does not necessarily imply unrestricted domain on L. Without specifying σ, however, nothing can be said about the structure of the domain of induced preferences. Given our focus, therefore, we make the conservative presumption that the domain of induced preferences over L is the unrestricted set of either weak or strong orderings, denoted $\mathrm{Q}(L)$ and $\mathrm{Q}^*(L)$, respectively. In what follows, we take the domain to consist of weak orders unless explicitly stated otherwise.

In sum, the formal representation of stages 1, 2, and 3 of an electoral system is given by a specification of the triple (β, γ, σ). The composition of these maps, $\beta \circ \gamma \circ \sigma$, thus characterizes the translation of voter preferences into policy outcomes through the electoral system.

As noted in the introduction, our principal aim is to establish results concerning monotonicity in electoral systems. To simplify the following arguments we distinguish two cases: single-member district ($\ell = 1$) and multimember district ($\ell > 1$). Consider these in turn.

Monotonicity in Single-Member Districts

By assumption, $\ell = 1$ so that $L = K$, and V_i is simply the set of orderings of candidates, K, competing for election. Thus, we can interpret a strategy by i, v_i, as a ranking of K; so the statement "$x\ v_i\ y$" means that x is ranked ahead of y according to i's strategy v_i.

DEFINITION 1. *Let* $\alpha : \mathrm{Q}(L)^n \rightarrow L$ *be an arbitrary function from voter preferences into legislatures. We say that* α *is monotonic if* $x = \alpha(\mathbf{v})$ *and* $\mathbf{v}'$ *is such that* $x\ v_i\ y$ *implies* $x\ v'_i\ y$, *for all* $i \in N$ *and for all* $y \in L$, *then* $x = \alpha(\mathbf{v}')$.

Suppose we apply this definition to an electoral rule γ. Then monotonicity for single-member districts requires that if a candidate is elected under a strategy profile **v** and if under a new strategy profile **v**′ every individual ranks that candidate at least as high as under **v**, then the candidate should still be elected. Thus, monotonic electoral rules respond positively to changes in an individual's stated rankings of candidates. This description corresponds precisely to the notion of process-oriented monotonicity alluded to in the introduction.

Now consider the composition $\beta \circ \gamma$; observe that if β is single-valued, then, just as with an electoral rule γ per se, $\beta \circ \gamma$ is a mapping from preference profiles into legislatures. Setting $\beta \circ \gamma = \alpha$ in definition 1 yields the requirement that if a candidate is elected under a preference profile **R**, and if under a new preference profile **R**′ every individual ranks that candidate at least as high as under **R**, then the candidate should still be elected. And this is exactly the notion of consequence-oriented monotonicity.

The first result, Saijo's Theorem, makes use of the concept that a function α: $\mathbf{Q}(L)^n \rightarrow L$ is *constant* if for all $\mathbf{R}, \mathbf{R}' \in \mathbf{Q}(L)^n$, $\alpha(\mathbf{R}) = \alpha(\mathbf{R}')$:

THEOREM 1. *If* α: $\mathbf{Q}(L)^n \rightarrow L$ *is monotonic, then* α *is constant (Saijo 1987).*

Hence, the requirement of monotonicity in an electoral rule is simply too stringent; the only electoral rule that satisfies process-oriented monotonicity is a trivial rule that selects the same legislature regardless of the individuals' voting decisions. Similarly, consequence-oriented monotonicity implies that the chosen legislature is never a function of the individuals' preferences if the behavioral rule $\beta(.)$ is single-valued. For example, if $\beta(.)$ is the identity function (i.e., voters behave "sincerely"), the translation of voter preferences into legislatures will be nonmonotonic.

An important feature supporting Saijo's Theorem is the possibility of individual indifference over alternatives in L. For some electoral systems, however, this may not be appropriate; individuals may have only strict preferences over the alternatives they confront. So suppose we restrict attention to the set of linear orders over L, $\mathbf{Q}^*(L)$. Say that a function α: $\mathbf{Q}^*(L)^n \rightarrow L$ is *dictatorial* if there exists an $i \in N$ such that for all profiles $\mathbf{P} \in \mathbf{Q}^*(L)^n$, $\alpha(\mathbf{P})\ P_i\ \lambda$ for all $\lambda \in L \setminus \alpha(\mathbf{P})$. Also say that α satisfies *citizen sovereignty* if for all $\lambda \in L$, there exists a profile $\mathbf{P} \in \mathbf{Q}^*(L)^n$ such that $\alpha(\mathbf{P}) = \lambda$.

THEOREM 2. *Suppose* $|L| \geq 3$. *If* α: $\mathbf{Q}^*(L)^n \rightarrow L$ *is monotonic and satisfies citizen sovereignty, then* α *is dictatorial (Muller and Satterthwaite 1977).*

Thus, as with theorem 1, requiring either process-oriented or consequence-oriented monotonicity (with $\beta(.)$ single-valued) yields an essentially negative result.

A key element to both the results above is that the mapping α is single-valued. By definition, an electoral rule γ is necessarily single-valued; therefore, theorems 1 and 2 together imply that no reasonable electoral rule (i.e., nondictatorial and nonconstant) can satisfy process-oriented monotonicity. Similarly, if the behavioral model β is single-valued, the same conclusion applies to consequence-oriented monotonicity. However, many behavioral models are not necessarily single-valued, in which case the composite map $\beta \circ \gamma$ taking preference profiles into legislatures will not be single-valued and the theorems do not apply.[3] Hereafter, we relax the assumption $\beta(.)$ is single-valued and focus on a particular multivalued behavioral model, the Nash equilibrium correspondence.

DEFINITION 2. *A correspondence* μ: $\mathbf{Q}(L)^n \rightarrow\!\!\rightarrow L$ *is monotonic if* $x \in \mu(\mathbf{R})$ *and* $\mathbf{R}'$ *is such that* $x\ R_i\ y$ *implies* $x\ R_i'\ y$, *for all* $i \in N$ *and all* $y \in L$, *then* $x \in \mu(\mathbf{R}')$.

Note that definition 1 above is a special case of definition 2 when μ is single-valued, that is, a function.

DEFINITION 3. *A strategy profile* $\mathbf{v}^*$ *is a Nash equilibrium if and only if for all* $i \in N$ *and all* $v_i \in V_i$, $\gamma(\mathbf{v}^*)\ R_i\ \gamma(v_i, (v_k^*)_{k \in N \setminus \{i\}})$.

Let $\hat{\beta}(\mathbf{R}; \gamma)$ denote the set of Nash equilibrium strategies given the electoral rule γ and true voter preferences $\mathbf{R} = (R_i)_N$ over L.

In contrast to previous conclusions concerning monotonicity, the next result shows that given the behavioral model $\hat{\beta}(.)$, *all* electoral rules are monotonic when considering the relationship between true preferences and the chosen legislatures.

THEOREM 3. *For all electoral rules γ, the correspondence $\beta(.;\gamma)\circ\gamma : \mathrm{Q}(L)^n \twoheadrightarrow L$ is monotonic.*

Proof. Fix $\mathbf{R}$ and γ and let $\lambda \in L$ be supported as an equilibrium outcome by the strategy profile, $\mathbf{v}^*$. Let $\mathbf{R}'$ satisfy the antecedent in the definition of a monotonic correspondence; the claim is that $\mathbf{v}^*$ is an equilibrium for $\mathbf{R}'$. If not, there exists a v_i, $i \in N$, such that $\sim[\gamma(\mathbf{v}^*)\ R_i'\ \gamma(v_i, (v_k^*)_{k\neq i})]$. But this implies $\sim[\gamma(\mathbf{v}^*)\ R_i\ \gamma(v_i, (v_k^*)_{k\neq i})]$, a contradiction. QED

Hence, the Nash equilibrium correspondence generates consequence-oriented monotonicity for *all* electoral rules.

From theorem 3, it is immediate that results concerning the nonmonotonicity of STV and similar rules depend critically on the assumption that voters faithfully report their true preferences, independent of any strategic or other considerations. In particular, if γ is not monotonic, as is the case with nonconstant (theorem 1) or nondictatorial (theorem 2) electoral rules, then the following corollary shows that this assumption is inconsistent with optimizing behavior on the part of the voters:

COROLLARY 1. *If (V, γ) is such that γ is not monotonic, then there exists a preference profile $\mathbf{R} = (R_1, \ldots, R_n)$ such that $v_i = R_i$, for all i is not a Nash equilibrium (Muller and Satterthwaite 1977).*

Putting the results together yields our main conclusion for single-member districts: given that constant and dictatorial electoral rules are deemed unreasonable, either *no* reasonable electoral rule is monotonic or, relative to $\hat{\beta}(.)$, *all* electoral rules are monotonic.

Monotonicity in Multimember Districts

By definition of a multimember district, $\ell > 1$. Consequently, although voters in N have well-defined induced preferences $(R_i)_N$ over L, such preferences are over *sets* of candidates of cardinality $\ell > 1$ rather than over individual candidates per se. Electoral rules such as STV, however, require voters to report rankings of individual candidates. Hence, $V_i = \mathrm{Q}(K) \neq \mathrm{Q}(L)$. Examples of the nonmonotonicity of, for example, STV, for multimember districts (e.g., Doron and Kronick 1977) use a notion of monotonicity related to that of definition 2 above. Specifically, if $\mathbf{v}$ and $\mathbf{v}'$ are lists of orderings of candidates in K and if they are such that for all voters i, $x\ v_i\ y$ implies $x\ v_i'\ y$, then the collection of candidates chosen under $\mathbf{v}$ coincides with that chosen under $\mathbf{v}'$.

There are two difficulties with interpreting nonmonotonicity results based on this notion. The first is related to the distinction between process-oriented and consequence-oriented monotonicity. The second difficulty is that even allowing for a process-oriented judgment, this notion of monotonicity is inappropriate. The reason is that in multimember districts electoral rules do not, by definition, select single candidates but, rather, groups of candidates. Therefore, the relevant domain for any criterion such as monotonicity should be the set of individual preferences over such groups; these are, after all, the objects of choice. The mere fact that one candidate has risen in some individual's ranking does not necessarily tell us anything about changes in that individual's rankings over the relevant groups of candidates, that is, over L. Indeed, given our model, it is evident that voters do not even have well-defined preferences over candidates when district magnitude exceeds one. Basic preferences are over outcomes, X; and these induce preferences over multimember legislatures in L via the legislative outcome function, σ.

To illustrate the last point, suppose the district magnitude is 2 ($\ell = 2$), and suppose voter i ranks candidate a above candidate b. What does this imply about voter i's preferences over electoral outcomes, given that the district elects not one candidate but two? We would claim that any implication consistent with this requires a restriction on voter i's preferences over pairs of candidates; specifically, for any other candidate c, the pair of candidates (a, c) is preferred to the pair (b, c).

In some circumstances the preceding restriction may be legitimate; yet it is a substantive restriction on individuals' preferences over electoral outcomes at the district level. For instance, in the double-member district example, let $K = \{a, b, c, d\}$ so that $L = \{(a, b), (a, c), (a, d), (b, c), (b, d), (c, d)\}$. Suppose the four candidates $\{a, b, c, d\}$ are associated with the points $\{1/8, 4/8, 5/8, 1\}$ respectively in the one-dimensional policy space $X = [0, 1]$ and suppose voter i has symmetric single-peaked preferences $\geq_i$ on X with ideal point 1/2. Finally, suppose the legislative outcome function is $\sigma(x, y) = [x + y]/2$, for all elected legislatures, $(x, y) \in L$. Thus, at the legislative stage, the final policy outcome is the compromise given by the midpoint of the elected candidates' positions. Then it is easy to check that the voter has induced preferences R_i over L such that (a, d) is strictly preferred to (b, d), and (b, c) is strictly preferred to (a, c). But this violates the necessary restriction on preferences over K; the first ranking can only be rationalized at the individual candidate level by saying "i prefers a to b," while the second ranking can only be rationalized by saying the converse. Thus, voter i is incapable of providing a ranking of individual candidates that reflects i's preferences over electoral outcomes (i.e., pairs of candidates).

Notice that there exists a "natural" ordering of the candidates for voter i, given by their policy positions in X; that is to say, $b >_i c >_i a >_i d$. Were the district magnitude equal to one, this ordering would certainly constitute i's preferences over L. However, it is immaterial, given that the magnitude is two.

To avoid the difficulty with STV—or indeed with any multimember electoral rule where the strategy sets are ranking of individual candidates—one can simply require voters to report rankings of ℓ-tuples of candidates (i.e., rankings of L). But then all electoral rules are observationally equivalent to single-member electoral rules, where now "a member" is an element of L.

Given that this modification is implemented, theorems 1 and 2 apply: any reasonable electoral rule fails the process-oriented notion of monotonicity. Additionally, such rules fail consequence-oriented monotonicity when the behavioral rule $\hat{\beta}$ is single-valued. However, given β (i.e., the Nash equilibrium correspondence), the set of equilibrium outcomes will always respond positively to changes in voter preferences regardless of the electoral rule.

Conclusion

We have focused on the criteria of monotonicity and argued that as a normative critique the nonmonotonicity of electoral rules and systems has no bite. In particular, in any reasonable electoral system, outcomes will always respond positively to changes in voter preferences (given the Nash behavioral hypothesis) while not necessarily responding positively to changes in voters' reported preferences.

Finally, we emphasize that attempting to evaluate electoral systems by focusing exclusively on the electoral stages (stages 1 and 2) at the expense of the legislative stage (stage 3) is clearly inappropriate. Individuals have preferences over policy outcomes, and these induce preferences over the objects of choice (candidates or sets of candidates) through the legislative outcome function. The optimal voting decision for an individual in any election is therefore one that best promotes that individual's most preferred final outcome. Consequently, while an electoral rule may fail to satisfy a variety of appealing criteria with respect to recorded voting decisions, there is no reason to suppose the electoral system as a whole is not representative of individuals' policy preferences. Only through explicit consideration of all three stages can we have any confidence in judgments about any given electoral system's efficacy.

Notes

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1. This is in contrast to the literature on the existence of transitive, or acyclic, social choice rules.

2. Although most commonly employed electoral mechanisms do not require voters to report complete orderings, they can be modeled *as if* they do. For instance, plurality voting (with strict orderings) would simply ignore everything but the top-ranked alternative. Similarly, approval voting (with weak orderings) would assign a value of one to every alternative in the top-ranked set and zero to all remaining alternatives.

3. It is important to note that if $\beta(.)$ is set-valued, this does not imply that more than one candidate is elected to the legislature. Rather if $\mathbf{v}, \mathbf{v}' \in \beta(.)$, this says that for some strategy profile $\mathbf{v}$, $\gamma(\mathbf{v})$ is selected, that for a distinct profile $\mathbf{v}'$, $\gamma(\mathbf{v}')$ is selected, and that both $\mathbf{v}$ and $\mathbf{v}'$ are consistent with the behavioral rule $\beta(.)$.

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GOVERNMENT PARTISANSHIP, LABOR ORGANIZATION, AND MACROECONOMIC PERFORMANCE

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Governments of the Left and Right have distinct partisan economic policies and objectives that they would prefer to pursue. Their propensity to do so, however, is constrained by their desire for reelection. We argue that the ability of governments to further their partisan interests and preside over reelectable macroeconomic outcomes simultaneously is dependent on the organization of the domestic economy, particularly the labor movement. We hypothesize that there are two different paths to desirable macroeconomic performance. In countries with densely and centrally organized labor movements, leftist governments can promote economic growth and reduce inflation and unemployment. Conversely, in countries with weak labor movements, rightist governments can pursue their partisan-preferred macroeconomic strategies and achieve similarly beneficial macroeconomic outcomes. Performance will be poorer in other cases. These hypotheses are supported by analysis of pooled annual time series data for 16 advanced industrial democracies between 1967 and 1984.

There is considerable debate in the political economy literature over the impact of government partisanship on economic policies and outcomes among the advanced industrial democracies (Garrett and Lange 1989; Jackman 1986). Most scholars agree that governments dominated by parties of the Left and Right have distinct partisan economic objectives that they would prefer to pursue, ceteris paribus. However, there is much disagreement about the ability of governments to further these preferences. Two broad lines of argument can be delineated. On the one hand, many assert that governments can be expected to pursue partisan economic strategies consistently, because these further the interests of the governments' core political constituencies (organized interests, activists, and voters). Leftist governments are expected to intervene extensively in the economy to alter market outcomes and redistribute wealth and risk in ways favorable to the lower socioeconomic strata. Governments of the Right are expected to pursue less-interventionist strategies that benefit capital and the more wealthy (Hibbs 1977, 1987; Kirschen 1964; Lipset 1983). Other studies, on the other hand, highlight political constraints on the ability of governments to act on their partisan preferences.[1] In plurality electoral systems, Downs's (1957) spatial models of electoral competition suggest that all governments should pursue policies and outcomes that appeal to the median voter, regardless of their partisanship. In multiparty proportional representation systems, the dynamics of coalition, compromise, and consensus are said to mitigate partisan effects (Jackman 1986).

The expectations for macroeconomic outcomes generated by these two conceptions of government behavior are very different. Those emphasizing the incentives for governments to pursue partisan strategies typically argue that mixes of inflation and unemployment are significantly affected by which parties govern.[2] Governments dominated by leftist parties are assumed to concentrate on lowering the level of unemployment even at the risk of incurring higher inflation rates (leftist governments, hence, are unemployment-sensitive); rightist governments are seen to focus on lowering the inflation rate even if such action may produce higher unemployment (they are inflation-sensitive).[3] In marked contrast, those concentrating on the constraints against partisan effects predict that macroeconomic outcomes (including economic growth, in addition to inflation and unemployment) will not be systematically affected by who governs.

Empirical tests of these hypotheses have been inconclusive. Hibbs (1977) argued that a partisan Phillips curve was present among the advanced industrial democracies, but his original findings have been disputed (Beck 1982; Cameron 1984; Chappell and Keech 1988; Hibbs 1987). Furthermore, there is disagreement about the impact of partisanship on economic growth (Garrett and Lange 1989; Hicks 1988; Hicks and Patterson 1989; Jackman 1987, 1989; Lange and Garrett 1985, 1987). We argue that much of this debate over the macroeconomic consequences of government partisanship is based on a misspecified model of the political economy of capitalist democracy. The argument can be stated briefly.

We begin with a very simple understanding of government strategy. Governmental objectives are considered to be lexically ordered: all governments would prefer, ceteris paribus, to pursue their partisan distributive objectives; however, they will do so consistently only when such behavior does not harm their prospects for reelection. With respect to the economy, this reelection constraint is manifest primarily in the need for governments to preside over a desirable mix of macroeconomic outcomes, such as higher rates of growth and lower rates of inflation and unemployment.

We argue that the ability of governments to further their partisan interests and preside over reelectable macroeconomic outcomes *simultaneously* is significantly affected by the organization of the domestic economy, particularly the labor movement.[4] Two distinctive paths to desirable macroeconomic performance are hypothesized. On the one hand, in countries with densely and centrally organized ("encompassing") labor movements, the participation of leftist parties in government can be expected to promote economic growth and reduce inflation and unemployment. The more encompassing the labor movement, the more likely the policies preferred by leftist governments will provide incentives for unions voluntarily to restrain their market militancy. In turn, this behavior can be anticipated to increase the efficacy of interventionist policies in inducing capital to invest productively in the national economy. Under such conditions, leftist governments can be expected *consistently to pursue partisan strategies*, and these are hypothesized to generate better outcomes across all facets of economic performance.

On the other hand, where labor movements are much weaker, rightist governments can be expected to pursue their preferred strategies, which are likely to have similarly beneficial consequences for macroeconomic outcomes. The lesser the ability of unions to organize collective responses to market disciplines, the greater the efficacy of the Right's preferred policies in stimulating performance by reducing government interventions in the economy and by allowing market forces to operate.

Finally, in less coherent constellations of political and organizational conditions (where rightist governments do not encounter very weak labor unions or leftist governments are not allied with encompassing labor movements) governments will find it difficult consistently to pursue their partisan-preferred strategies, and political economic instability and poorer economic performance can be expected.

In the remainder of the paper, we develop the theoretical argument, present empirically testable hypotheses derived from this argument, discuss the methodologies we use to examine these hypotheses, report the results, and relate them to broader issues in the literature.

The Strategic Interaction of Governments, Labor, and Capital

Many studies on the macroeconomic consequences of government partisanship (both those arguing for partisan distinctiveness and those emphasizing political pressures for convergence) assume that economic processes—and the effects of government policies on them—are similar across the advanced industrial democracies. This assumption, however, recently has been challenged by those focusing attention on the impact of domestic structural factors, particularly in the labor market (Blanchard and Summers 1986; Bruno and Sachs 1985; Calmfors and Driffill 1988; Cross 1988; Olson 1982). As Bruno and Sachs conclude:

> It would seem only natural that a theory for a country's (or several countries') response (to economic shocks) can only be formulated if one takes its specific institutional or structural features into consideration. However, macrotheory, whether Keynesian or monetarist, has for a long time tended to consider one and the same basic model as applicable to all economies. . . . Our analysis highlights a third structural element—the importance of incorporating the structure of labor markets explicitly into macroeconomic theory that is applied across industrial economies. (1985, 274)

Sensitivity to the importance of labor market structures has led some to propose that the relationship between labor organization and macroeconomic outcomes is "U-shaped" (Calmfors and Driffill 1988). When labor movements are low in "encompassment" (the density of unionism and the centralization of peak associations; see Olson 1982), they are considered to have little market power or strategic capacity. Their impact on economic processes is small, and the economy is anticipated to perform well, in accordance with neoclassical economic models. As encompassment increases, the power of unions grows. They become better able to retaliate with increased wage and strike militancy to the imposition of market disciplines regardless of the negative externalities of such behavior. At very high levels of encompassment, however, unions potentially internalize such a high fraction of these costs that their leaders might decide voluntarily to adjust their strategies to promote, rather than to impede, overall macroeconomic performance.

But political factors are not considered in these economic studies. Unions' strategic calculations are presumed to depend solely on their internal organizational characteristics; they do not include expectations about government partisanship and policies. Furthermore, the studies do not entertain the possibility that organizational conditions might also have a significant impact on the efficacy of different economic policies and hence might influence the strategies governments actually pursue.

However, unions' expectations about the types of policies governments may pursue are likely to have significant consequences for the behavior of labor movements (Garrett and Lange 1990; Lange and Garrett 1985). If, for example, encompassing unions were to confront rightist governments (which they expect, prima facie, will not pursue policies facilitating the redistribution of wealth to workers and the reduction of social risk), it is unlikely that they would voluntarily restrain their militancy to increase national economic product (as Olson [1982] seems to assume). Similarly, the argument that very weak labor unions will be unable to subvert the operation of market forces could be undermined by the presence of sympathetic leftist governments that may cushion the effects of market mechanisms through restraints on employer discretion, or with extensive welfare and employment creation policies.

Furthermore, just as anticipated government policies may affect the behavior of labor movements, so might governments' expectations about labor's behavior influence the course of government policy. If governments dominated by leftist parties, for instance, can be confident that encompassing labor movements will restrain their behavior, the governments may be able to pursue their partisan preferences for more interventionist policies designed to increase the public provision of welfare and create employment opportunities without fears of setting off inflation and retarding growth (Scharpf 1984; Schott 1984).[5]

Finally, expectations about the behavior of labor and government may also affect business strategy. All else equal, capitalists may be less likely to invest in the domestic economy when they confront encompassing unions or when government is dominated by the Left. But the potential for a "virtuous circle" (Castles 1978) between leftist government and encompassing labor may generate a stable political economic environment in which business confidence in the medium term would be quite high, thus stimulating the efficient investment of profits. Conversely, business may choose not to invest in the national economy or may pursue short-term profitability at the expense of longer-term growth and competitiveness even if the Right dominates government (where it confronts powerful labor) or even if unions are very weak (when the Left governs). In both cases, the fear of volatile political and economic conditions would significantly undermine business confidence.

Three sets of relationships between government partisanship and macroeconomic outcomes can be derived from this conception of the strategic interactions between capital, governments and labor. The first two, in which governments of both the Left and Right can be anticipated consistently to pursue partisan strategies and preside over desirable macroeconomic outcomes, are stated briefly. More detailed exposition is required for the final scenario in which government strategy is likely to be less consistent and economic performance is expected to be poorer.

Leftist Governments That Are Allied with Encompassing Unions

While the Left's preference for more interventionist policies may allow it to maintain relatively full rates of employment, it is often argued that such a path to the reduction of unemployment invariably has undesirable consequences for inflation and economic growth. Where unions are encompassing, however, the pursuit of welfarist policies by leftist government is likely to generate voluntary labor restraint, which in turn should promote an environment conducive to capitalist investment. Furthermore, such restraint can also be expected to increase the efficacy of leftist policies designed to stimulate business investment that might otherwise have inflationary consequences: macroeconomic expansion (Castles 1978; Martin 1984; Scharpf 1984; Schott 1984) and more targeted supply side measures (Garrett and Lange 1990). Finally, under these conditions the support of labor unions for government policies designed to stimulate growth is likely to facilitate long-term capital investment. Thus, in cases with encompassing unions, it is hypothesized that leftist governments can consistently pursue their partisan-preferred economic policies and that these will have desirable macroeconomic consequences with respect to unemployment, economic growth, and inflation.

Rightist Governments That Encounter Very Weak Unions

The conventional criticism of the Right's preference for less interventionist economic policies is that successfully controlling inflation can only be achieved at significant costs with respect to unemployment and economic growth. Where unions are sufficiently weak and decentralized to be rendered unable effectively to retaliate against the imposition of market discipline, however, rightist policies may be consistent not only with price stability but also with higher rates of growth and with lower levels of unemployment. Economic conditions, hence, would more closely approximate those assumed in neoclassical economic prescriptions for less interventionist government strategies. Wages would be determined largely by supply-and-demand conditions, and successful entrepreneurs would be freer to invest their profits in response to market signals (relatively unaffected by government interventions or unions' behavior). Thus, under these circumstances, rightist governments can be expected consistently to pursue partisan strategies and preside over desirable macroeconomic outcomes.

Left Governments and Nonencompassing Unions, Right Governments and Encompassing Unions

It is implicit in these analyses that leftist policies would have deleterious macroeconomic consequences if pursued where labor movements are not encompassing and rightist policies would be ineffective in the presence of encompassing unions. But given the lexical priority of presiding over reelectable macroeconomic outcomes, it might be anticipated that governments would not pursue partisan strategies in such cases. Rather, they might merely emulate the policy strategies of their opponents.

There are good reasons to expect, however, that the conversion of leftist and rightist governments to the other's policies will frequently take some time, that is, governments will undertake midcourse U-turns away from partisan strategies rather than dropping them at the outset of their terms in office (Alt 1985; Garrett 1990). Over time, however, in cases where the organization of labor renders partisan strategies infeasible, leftist and rightist governments can both be expected to move away from their preferred policies toward those that more closely fit prevailing conditions.[6]

But such attempts to improve macroeconomic performance are unlikely to be particularly successful. Rightist governments can be anticipated to find it difficult to engender in unions the confidence required for voluntary labor restraint, while leftist governments might not be able sufficiently to alter capitalists' expectations to stimulate investment. Furthermore, in the eyes of capital and labor, inconsistencies between the preferences of the government's major constituencies and its reelection requirements are likely to reduce the credibility of the government as an economic manager; so, too, will a past history of midterm U-turns. It is unlikely that in such an environment of low credibility and uncertain expectations governments would be able consistently to generate desirable outcomes with respect to economic growth, inflation, or unemployment.

Hypotheses

These expectations about the interactive effects of government partisanship and labor organization on macroeconomic outcomes can be summarized in the following pair of interrelated hypotheses, displayed graphically in Figures 1 and 2.

HYPOTHESIS 1. *The more (less) encompassing the labor movement, the stronger the beneficial consequences for growth, inflation, and unemployment performance of more leftist (rightist) participation in cabinet government.*

In cases where unions are highly encompassing, leftist participation in cabinet government can be expected to be strongly and positively associated with better macroeconomic outcomes, whereas rightist government would tend to depress performance. As the strength of the labor movement declines, the positive impact of leftist government (and the negative effects of rightist administrations) is anticipated also to diminish. Where unions are very weak, leftist government is likely to have deleterious consequences for macroeconomic performance, whereas rightist control of cabinet can be expected to improve performance significantly.

HYPOTHESIS 2. *The stronger leftist (rightist) participation in cabinet government, the more powerful the beneficial consequences for growth, inflation, and unemployment performance of more (less) encompassing labor movements.*

In cases where leftist parties dominate

Figure 1. The Conditional Impact of Government Partisanship on Economic Performance

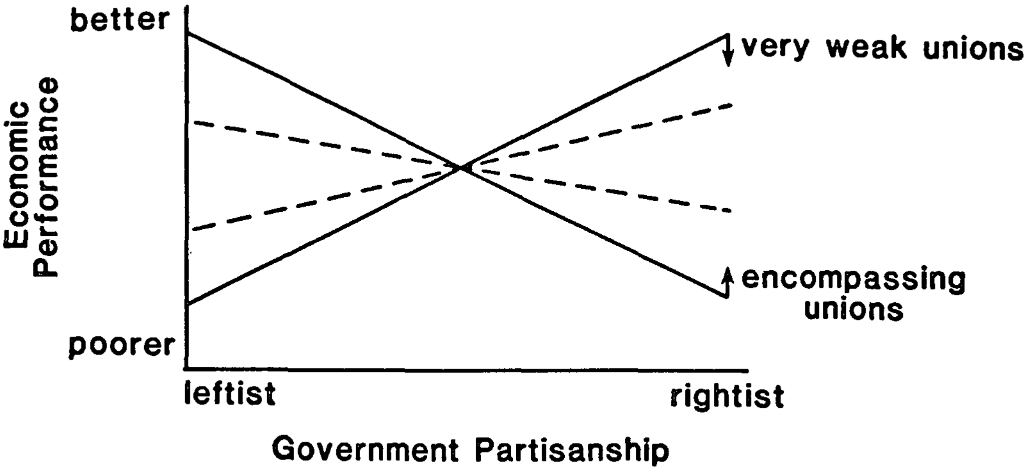
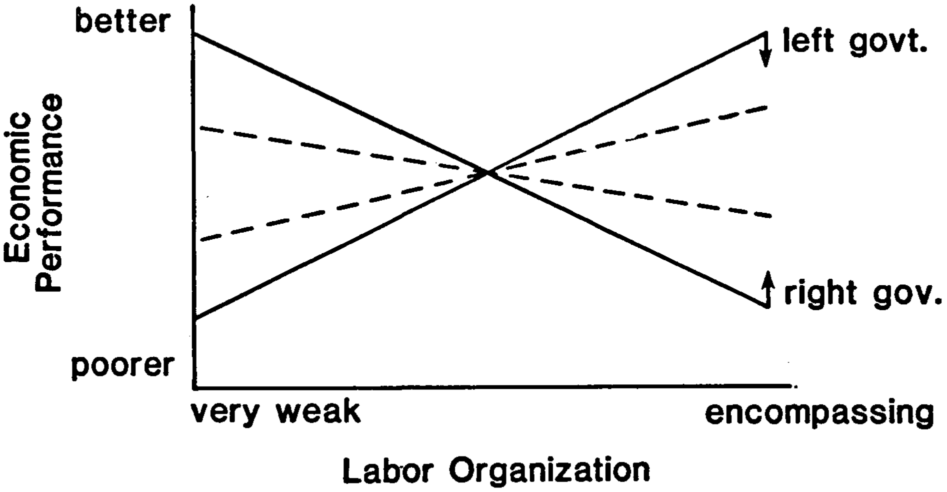


Figure 2. The Conditional Impact of Labor Organization on Economic Performance



government, increases in the organizational strength of labor movements can be expected to be strongly and positively associated with better macroeconomic outcomes, whereas decreases in labor power would tend to weaken performance. As the strength of leftist parties declines, the positive impact of more encompassing unions is also expected to decrease. Where parties of the Right are dominant, increasing labor union strength is likely to have deleterious consequences for macroeconomic performance, whereas the weakening of labor movements can be expected to improve performance significantly.

Methodology

In order to examine these hypotheses we have pooled time series data for the period 1967–84 across 16 advanced industrial democracies.[7]

Variables

Economic Performance. Three basic indicators of economic performance were examined: annual rates of gross domestic product growth, change in the annual rate of the consumer price index growth, and change in annual unemployment levels.[8]

Government Partisanship. Government partisanship is operationalized as the annual percentage of cabinet portfolios held by parties of the Left.[9] The coding of Left and non-Left political parties is that used by Cameron (1984), Lange and Garrett (1985), and Swank (1989).

Labor Organization. The degree of labor organization encompassment is measured by summing standardized scores for the density and centralization of union movements in each of the countries in the study (Lange and Garrett 1985, 805; also see Appendix).

Domestic Economic Processes. We do not here purport to construct sophisticated simultaneous equation models (containing variables such as investment, savings, and wage rates in addition to government policy outputs) that accurately reflect the complex effects of domestic economic processes on macroeconomic outcomes. For present purposes, simpler controls for these economic forces have been deployed within a single-equation framework: lagged values of the growth, inflation, and unemployment outcomes.[10] The statistical problems created by this procedure are discussed below.

Vulnerability to International Economic Conditions. A battery of variables was used to account for the impact on different national economies of demand-and-supply conditions in the international economy. The variables weight annual Organization for Economic Co-Operation and Development (OECD) economic growth rates (OECD demand), growth in OECD export prices (supply conditions within the OECD), and growth in OECD import prices (supply conditions external to the OECD), by the "openness" (exports as a percentage of gross domestic product) of each economy in the sample.[11]

The Pooled Time Series Model

Linear equations were estimated using pooled annual time series data to test the hypotheses. Pooling the data allows us to examine the interactive effects on economic performance of government partisanship and labor organization both over time and across countries. In doing so, we can circumvent weaknesses in much of the existing research, which typically concentrates either on aggregated cross-national data (Cameron 1984; Hicks 1988; Lange and Garrett 1985; Garrett and Lange 1989) or on time series data for individual countries (Alt 1985; Chappell and Keech 1988; Hibbs 1977, 1987).

The general form of the estimated equation is:

$$Y_{nt} = \alpha + \beta_1 Y_{nt-1} + \Sigma(\beta_k X_{knt}) + u_{nt},$$

where Y is one of three economic performance indices (the annual rate of economic growth, annual rate of change in the consumer price index inflation, or annual rate of change in unemployment levels); X_k is the set of exogenous variables; α is an intercept term; the subscripts $_n$ and $_t$ denote the country and year of observations, respectively; and u_{nt} is a disturbance term. With pooled time series data, however, it is difficult to develop an estimation method for such an equation that yields efficient, unbiased, and easily interpretable empirical results (Hsiao 1986; Judge et al. 1988; Kmenta 1971; Sayrs 1989; Stimson 1985).

First, the pooled time series design typically violates two assumptions underlying ordinary least squares estimation: that the disturbance terms have constant variance and that these disturbances are not correlated. Thus, close attention must be paid to the potential for autoregressive, heteroscedastic, and contemporaneously correlated disturbance terms. Violations of these assumptions are present in ordinary least squares analysis of our data. Examination of residual variances for the 16 countries on each dependent variable (and not controlling for autoregression or heteroscedasticity) indicates a considerable degree of nonconstant residual variance. Further, ARIMA analysis reveals that for the growth models, an AR(1) process depicts the disturbance term correlations for each country. For both inflation and unemployment, AR(1) fits the relationships between disturbance terms only after first-order differencing eliminates nonstationarity in each series.[12] Finally, it is probable that the disturbance terms from different countries in the same year or from different countries and different years are correlated (Sayrs 1989, 13).

Second, the inclusion of the lagged dependent variable produces inconsistent estimates when autocorrelation is also present (Johnston 1984, 362–63). To obtain consistent (but still biased) estimates, instrumental variables were created using the logic of two-stage least squares estimation (Hibbs 1974, 297): the dependent variables were regressed on lags of the original variables and a vector of lagged exogenous variables to produce instruments that were substituted for the actual values of the dependent variables on the right-hand side of each equation:[13]

$$Y_{nt} = \alpha + \beta_1 Z_{nt-1} + \Sigma(\beta_k X_{knt}) + e_{nt},$$

where Z_{nt-1} represents the instrument for the lagged dependent variable and e_{nt} is a disturbance term.

These assumptions and modifications permit the use of a multiple stage generalized least squares procedure to obtain parameter estimates and their standard errors.[14]

Interaction Models and Conditional Analysis

We tested our hypotheses about the interactive effects of government partisanship and labor organization with the following equation:

$$\begin{aligned} Y_{nt} = \alpha &+ \beta_1 Z_{nt-1} + \beta_2 \mathrm{LFCAB}_{nt} \\ &+ \beta_3 \mathrm{LORG}_{nt} + \beta_4 \mathrm{LFCAB}_{nt} \bullet \mathrm{LORG}_{nt} \\ &+ \Sigma(\beta_k \mathrm{INT}_{knt}) + e_{nt}, \end{aligned} \tag{1}$$

where LFCAB is leftist participation in cabinet government, LORG is the labor organization index, and INT stands for the battery of indicators of national vulnerability to international supply and demand conditions.

If this equation were to support the conditional relationships posited in hypotheses 1 and 2, we would expect the following pattern of coefficients: (1) in the economic growth models, the LFCAB and LORG coefficients should be negative, whereas those for the LFCAB × LORG interaction are anticipated to be positive (but smaller than the coefficients for each of its constituent parts); (2) in the inflation and unemployment models, the LFCAB and LORG parameter estimates should be positive, whereas those for the interaction term should be negative (and smaller).

This pattern of results would generate the predicted types of conditional relationships between government partisanship, labor organization, and economic performance predicted in the pair of hypotheses: positive associations for leftist (rightist) government where labor organization is encompassing (very weak) and positive parameter estimates for encompassing (very weak) unions where government is dominated by leftist (rightist) parties. In order to show why this is so, it is necessary to manipulate equation 1:

$$\begin{aligned} Y_{nt} = \alpha &+ \beta_1 Z_{nt-1} + \beta_3 \mathrm{LORG}_{nt} \\ &+ (\beta_2 + \beta_4 \mathrm{LORG}_{nt}) \mathrm{LFCAB}_{nt} \\ &+ \Sigma(\beta_k \mathrm{INT}_{knt}) + e_{nt}, \end{aligned} \tag{2}$$

or

$$\begin{aligned} Y_{nt} = \alpha &+ \beta_1 Z_{nt-1} + \beta_2 \mathrm{LFCAB}_{nt} \\ &+ (\beta_3 + \beta_4 \mathrm{LFCAB}_{nt}) \mathrm{LORG}_{nt} \\ &+ \Sigma(\beta_k \mathrm{INT}_{knt}) + e_{nt}. \end{aligned} \tag{3}$$

The terms in parentheses represent the conditional coefficients for LFCAB (equation 2) and LORG (equation 3), respectively. Standard errors were derived from the parameter estimate variance–covariance matrix of equation 1 (Friedrich 1982; Lange and Garrett 1985). Thus, in equation 2 for example, if β_2 were negative and β_4 were positive but smaller (by a factor of more than .41, the smallest value for LORG, but less than 3.62, its largest value), the conditional coefficient for LFCAB would be positive where LORG = 3.62 but negative if LORG = .41. Similarly, if in equation 3, β_3 were negative and

β_4 were positive but smaller by a factor of one hundred (complete leftist control of government), the conditional coefficient for LORG would be positive where LFCAB = 100 and negative where LFCAB = 0. This is what we would expect for the economic growth models.

Results

Overall Estimates

The results reported in Table 1 show that even when the effects of domestic and international economic forces were taken

Table 1. Parameter Estimates for the Pooled Time Series Models of Growth, Inflation, and Unemployment, 1967–1984

	Dependent Variable		
Independent Variables	Gross Domestic Product (GDP)	Consumer Price Index (CPI)	Unemployment (UNEM)
Intercept	3.76* (.20)	–.189* (.035)	.394* (.013)
Lagged dependent variable[a]	.166* (.015)	.098* (.009)	.308* (.006)
Vulnerability to OECD demand conditions[b]	.008* (.0002)	–.0009* (.00003)	–.001* (.00002)
Vulnerability to OECD supply conditions (export prices)[c]	.002* (.0001)	–.001* (.00001)	–.0002* (.00001)
Vulnerability to OECD supply conditions (import prices)[d]	–.005* (.0002)	.015* (.00004)	–.002* (.00004)
Labor organization index (LORG)[e]	–.911* (.082)	.058 (.036)	–.059* (.006)
Leftist participation in cabinet government (LFCAB)[f]	–.027* (.001)	.018* (.0003)	.005* (.0004)
LORG × LFCAB	.014* (.0004)	–.010* (.00009)	–.002* (.0001)
Degrees of freedom	232	232	232
Adjusted R^2	.40	.28	.24
F-ratio	34.2	16.6	13.5

Note: All entries are generalized least squares estimates, with standard errors in parentheses. GDP is the annual rate of growth in domestic output. CPI and UNEM are first differences of the annual rate of price inflation and the annual level of unemployment respectively.

[a] Instrumental variables.

[b] Annual OECD economic growth rates weighted by the "openness" (exports/GDP) of the 15 countries in the sample.

[c] Annual growth in OECD export prices weighted by openness.

[d] Annual growth in OECD import prices weighted by openness.

[e] The sum of standardized scores for the density and centralization of labor movements (Lange and Garrett 1985, 805).

[f] Annual percentage of cabinet portfolios held by leftist parties.

*$p < .01$, one-tailed test.

Table 2. The Conditional Impact of the Participation of Leftist Parties in Cabinet Government on Economic Performance

Labor Organization Score	Conditional Left Cabinet Parameter Estimates		
	GDP	Inflation	Unemployment
3.62 (Sweden)	.024* (.0007)	-.018* (.0001)	-.0022* (.0002)
2.80 (Finland)	.012* (.0005)	-.010* (.0001)	-.0005* (.0001)
1.73 (Germany)	-.003* (.0007)	.0007* (.0001)	.0015* (.0002)
.41 (Japan)	-.021* (.0011)	.014* (.0002)	.0042* (.0003)

Note: Standard errors are in parentheses.

*$p < .01$, one-tailed test.

into account, the political and organizational variables remained powerful influences on macroeconomic outcomes.[15] As anticipated, the LORG and LFCAB coefficients were negative in the economic growth equations, and positive for the change in inflation and unemployment models (with the exception of the LORG estimates in the unemployment equation). Conversely, the parameter estimates for the LORG × LFCAB interaction terms were positive for growth and negative for inflation and unemployment performance. Furthermore, almost all of these coefficients were significant at the $p = .01$ level.

A series of tests for the reliability and robustness of the equations was also undertaken. Analysis of residual variances and correlations indicated that autocorrelation and heteroscedasticity were effectively controlled by the estimation procedure. Furthermore, sequential deletion of single-country time series from the analysis revealed considerable stability in the parameter estimates and hence that individual cases were not unduly influential. In sum, these tests increase our confidence in the robustness of the results.[16]

The Conditional Effects of Government Partisanship

The conditional effects of government partisanship (at different levels of LORG) on economic performance are summarized in Table 2.[17] These results accord very closely with our theoretical expectations (and all the conditional coefficients were significant at the $p = .01$ level or better). In countries with highly encompassing labor movements (such as Sweden), leftist participation in governments was strongly associated with higher rates of growth and with slower increases in inflation rates and in unemployment levels. These beneficial consequences of leftist government were smaller in countries where labor movements, while quite strong, were not so encompassing (e.g., Finland).

Leftist participation had deleterious consequences for economic performance, however, in countries where union movements were less centralized and less densely organized. In the most extreme case (Japan, with the weakest union movement in our study), the costs of possible leftist participation were almost as strong as the benefits of leftist power with very encom-

Table 3. The Conditional Impact of Labor Organization on Economic Performance

	Conditional Labor Organization Parameter Estimates		
% of Cabinet Leftist	GDP	Inflation	Unemployment
100	.489** (.095)	-.942** (.040)	-.259** (.017)
66	.013 (.089)	-.602** (.038)	-.191** (.012)
33	-.449** (.085)	-.272** (.037)	-.125** (.009)
0	-.911** (.083)	.058* (.036)	-.059** (.006)

Note: Standard errors are in parentheses.
*$p < .05$, one-tailed test.
**$p < .01$, one-tailed test.

passing unions with respect to growth and changes in inflation and almost twice as large again with respect to changes in unemployment. These results could also be interpreted in terms of the impact of rightist government on performance—positive where unions were very weak, smaller as union strength was increased, and negative where labor movements were encompassing.

The magnitude of these effects of government partisanship was substantial, especially with respect to growth rates and changes in inflation rates. In a country with a labor movement as encompassing as that in Sweden, for instance, an election in which the Left swept to power (changing the LFCAB score from zero to one hundred) would have increased annual economic growth by 1.5 percentage points and decreased the rate of inflation change by 2.2 points, all else equal. Conversely, an election that removed the Right from power in a country with a labor movement similar to Japan's would have decreased growth by 2.2 points and increased the change in annual inflation by 2.5 points.

The impact of government partisanship on changes in unemployment levels were smaller. If the Left was removed from office in a country such as Sweden, for instance, the rate of growth in unemployment would have increased by .21 points. Similarly, the onset of a leftist government in a country with Japan's type of labor movement would only have worsened unemployment performance by .40 points. Nonetheless, even these results indicate that the conditional and conjunctural impact of government partisanship was substantial, despite the "stickiness" of unemployment levels (Blanchard and Summers 1986).

The Conditional Effects of Labor Organization

The conditional impact of labor organization on economic performance is summarized in Table 3. The growth and changes in inflation equations conform to our theoretical expectations (and the conditional parameter estimates were almost always significant at the $p = .01$ level). When governments were dominated by leftist parties, economic growth rates were larger and increases in inflation rates were smaller, the more encompassing the labor movement. These associations weakened as leftist power declined; and labor organization led to slower rates of economic growth and larger increases in inflation when the Left had little or no power in cabinet governments. This implies that weak union movements were conducive to better economic performance when the Right dominated government.

The conditional impact of labor organization on changes in unemployment differed from our theoretical expectations. Countries with more encompassing unions were always associated with slower increases in unemployment, regardless of which parties governed. Nonetheless, it is apparent that leftist participation in government magnified the beneficial consequences of labor organization. The LORG conditional coefficient was five times larger when governments were completely controlled by leftist parties than when these parties were wholly excluded from government.[18]

In sum, these results indicate that encompassing organizations were conducive to better economic performance when the Left was politically powerful. Weaker union movements had desirable consequences for growth and inflation when governments were dominated by rightist parties, but more encompassing labor movements were beneficial for unemployment irrespective of political conditions.

Discussion

Three basic conclusions about the impact of government partisanship and economic organizations on macroeconomic performance can be reached. First, the effects on performance of political conditions and organizational structures are mutually contingent. Leftist governments (encompassing unions), for example, have beneficial consequences for growth, inflation, and unemployment only where unions are encompassing (where the Left dominates cabinet government). Second, this implies that two configurations of political and economic organizational conditions are supportive of desirable macroeconomic outcomes. Performance is considerably better under more "coherent" configurations (the Left dominant in countries with encompassing unions and the Right in countries with very weak labor movements) than in more incoherent cases. Third, the contingent nature of the effects of government partisanship on economic performance has powerful implications for the debate over the economic effects of who governs. No general predictions of partisan distinctiveness or convergence (such as those made by the antagonists in the literature) can be sustained because the opportunities faced by governments of the Left and the Right are sharply affected by organizational conditions in labor markets. Where leftist or rightist parties govern in coherent systems, they can both further their partisan interests and help generate reelectable macroeconomic outcomes. If they govern in incoherent contexts, however, they may be unable to satisfy either objective —certainly not both. As a result, studies seeking to test the relative power of the two interpretations that fail to take into account the effects of domestic organizational structures are likely to be inconclusive.

The results reported also shed light upon a number of more specific points. It is apparent that the recent trend in the economics literature to consider the underlying structural and institutional context in which economic processes operate is well founded (Bruno and Sachs 1985; Calmfors and Driffill 1988). These studies can be bolstered, however, by the inclusion of political conditions in the strategic calculuses of economic actors.

In addition, doubts about "the politics of growth" raised against cross-national studies fail to apply when the more systematic pooled time series analysis is used (Jackman 1987, 1989). Furthermore, the inflation results we report show stronger and more consistent effects for political and organizational conditions than have previous aggregated cross-sectional studies (Cameron 1984).

Finally, the unemployment results are consistent with recent speculations about structural "hysteresis" (Blanchard and Summers 1986). However, nonaccelerating inflation rates of unemployment (NAIRUs) can be expected to vary considerably across countries according to their respective levels of labor movement organization. Shocks raising unemployment are likely to dissipate more rapidly in countries with more encompassing labor organizations, irrespective of who governs.

In conclusion, we would like to identify two implicit arguments that require further empirical investigation. First, the interaction between government partisanship and labor organization should have significant consequences for cycles in economic policies and performance within electoral periods. Alt (1985), Alesina (1989), and Alesina and Roubini (1990) all argue that the effects of changes in government partisanship should only have transitory effects on economic outcomes at the beginning of an electoral period. However, our argument suggests that pronounced cycles are likely to obtain only under more incoherent political economic conditions (Alvarez, Garrett, and Lange 1990; Swank 1989).

Second, we have made clear assumptions about the behavior of government, labor, and capital under different constellations of political and economic conditions. We assume, for example, that the desirable outcomes generated in left-coherent political economies are a product of interventionist government strategies that promote voluntary labor restraint and capital investment and that wage, strike, and investment rates respond to these policies. Similarly, we assume that when the Right governs in countries with very weak labor movements, far less interventionist policies are pursued, which in turn also restrain wage and strike militancy and stimulate investment by heightening the disciplining effects of market forces. In further work, it will be important to explore more carefully the mechanisms linking partisanship to performance, including the microeconomic institutions that are associated with the broader patterns we have identified (Soskice 1990; Streeck 1989).

These arguments about partisan cycles and about the paths from government partisanship to final economic outcomes have yet to be examined.

Appendix

Data used to construct our measure of labor organization strength were taken from Cameron, 1984, Table 7.6. The density of union movements is the average percentage of the total labor force unionized. The centralization of union movements is the average of Cameron's scores for organizational unity of labor, the power of confederations in collective bargaining, and the scope of collective bargaining. The labor organization index is the sum of the standardized density and centralization scores. The values these take for each country are shown in Table A-1.

Table A-1. Degree of Labor Organization Encompassment

Country	Labor Organization Index	Density	Centralization
Sweden	3.62	70	.83
Norway	3.46	65	.83
Austria	3.19	50	.93
Denmark	2.81	54	.70
Finland	2.80	47	.80
Belgium	2.77	55	.67
United Kingdom	1.93	45	.43
Netherlands	1.89	28	.67
Ireland	1.80	32	.57
Australia	1.77	40	.43
West Germany	1.73	32	.53
Italy	1.58	41	.33
Canada	.98	27	.27
United States	.86	21	.30
France	.81	24	.23
Japan	.41	16	.17

Notes

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1. There is also a large literature on the constraints imposed on government autonomy by conditions in the interdependent international economy. This literature has found that partisan separation between governments of the Left and the Right in the past 20 years has been manifest primarily with respect to supply side strategies for promoting competitiveness in world markets and adjustments to changes in them (Bosworth and Rivlin 1987; Garrett and Lange 1990; Katzenstein 1985).

2. Partisan differences with respect to public policy outputs, such as welfare, taxation, and fiscal and monetary policies have also been studied (Esping-Andersen and Korpi 1984; Garrett and Lange 1990; Hicks and Swank 1984; Swank 1989; Woolley 1984).

3. Some studies have not included inflation in this type of analysis because it is not clear that the costs of higher inflation fall consistently and disproportionately on the wealthy (Hibbs 1987; Piachaud 1978). Rather, they concentrate on unemployment and argue simply that leftist governments prefer lower to higher unemployment, whereas rightist governments favor the converse (Alesina 1989; Alt 1985).

4. It can also be argued that other domestic organizational conditions—such as the autonomy of central banks (Alesina 1989; Scharpf 1984) or the organization of business interests (Streeck and Schmitter 1985)—are likely to have a considerable impact on macroeconomic outcomes.

5. The conception of strategic government developed here is consonant with the large literature on economic retrospective voting (Fiorina 1981; Kiewiet 1983). Since all voters are vitally concerned with overall economic prosperity, governments that preside over poor macroeconomic outcomes do so at considerable electoral peril. Thus, governments should only be expected consistently to pursue partisan objectives where these are compatible with desirable macroeconomic performance.

6. However, actions by a partisan government might bring the structure of the labor market more closely in line with the government's policy preferences. The Thatcher government's attempts to undermine the power of organized labor in Britain provide a recent example of this strategy. Unfortunately, reliable statistical data on changes in labor market structure are not available. A survey recently conducted by one of us on quantitative indicators of labor movement structures in the advanced industrial democracies showed that almost all authors invoke the same few sources. As a result, we have assumed that these characteristics were invariant within countries during the 1967–84 period (although it would be preferble to use time series data were reliable cross-national series available).

7. The first year for which consistent time series data are available for all the variables we wish to examine is 1967. The 16 countries in the study are Australia, Austria, Belgium, Canada, Denmark, Finland, France, Ireland, Italy, Japan, the Netherlands, Norway, Sweden, the United Kingdom, the United States, and West Germany. We have not included Switzerland in our sample for a number of reasons. For one thing, the composition of the Swiss executive is not closely related to electoral outcomes. Thus, we cannot examine the differential effects of electorally linked partisan governmental policy strategies on macroeconomic outcomes. For another, across the period of our analysis the partisan composition of the Swiss government has remained almost constant. Combined with an economic time series of exceptionally low variance (and a constant term for the labor market structure), placing the Swiss case in our sample would induce a substantial degree of collinearity among many of our independent variables. We have also excluded New Zealand, since no comparable labor organization data exist.

8. The change data have been used because strong intertemporal correlations within countries in rates of inflation and in levels of unemployment rendered their analysis statistically intractable and theoretically uninteresting. For more discussion, see the next section concerning the pooled time series model. All economic data are from Organization for Economic Co-Operation and Development, 1987.

9. These data were made available to us by Duane Swank. We used the variable LEFTC in our analyses.

10. It also has been argued that long-run political economic conditions influence contemporary macroeconomic outcomes—wars, revolutions, medium-term government policy packages and the like (Castles 1989; Castles and Dowrick 1988). In turn, this suggests that lags of more than one year in the dependent variables may be warranted. Our specification, however, captures the notion that the large effects of factors such as government demand side policies are felt in the short term.

11. This specification delineates international effects on domestic outcomes more precisely than do the oil dependence variables that have often been used in analysis of post-1973 economic performance (Garrett and Lange 1986; Hicks 1988). The procedure used here is similar to that of Alt (1985), but we extend the analysis beyond the impact of international demand on domestic unemployment. We have not used trade-weighted measures of supply-and-demand conditions (deployed by Alt), because they are cumbersome to generate and imprecise.

12. Thus, we only report models that use changes in inflation and unemployment as dependent variables. We briefly discuss the nondifferenced results in n. 16, but these estimates have poor statistical properties.

13. Aggregate savings, real gross fixed capital formation, private consumption, and residential construction were used as exogenous variables. The data were taken from OECD sources.

14. In this procedure, ordinary least squares was used to calculate a vector of residuals from which the autoregressive parameter is derived. This parameter is used to transform the observations so that they are nonautoregressive. The residual vector is then recalculated with ordinary least squares, enabling consistent estimation of the variance-covariance matrix of the disturbance term vector. This allows us consistently to estimate the Ω matrix that is then used in the estimation of parameters and their standard errors (Kmenta 1971, 512–14). The estimates were obtained using the SAS PROC TSCSREG procedure and the PARKS subroutine. Due to the use of instruments for the lagged dependent variables, the standard errors estimated initially were biased. To alleviate this bias, standard errors and summary statistics for each equation were calculated separately. Details of these statistical methods are available from us upon request.

15. Specifically, domestic economic processes (the lagged instruments) and the vulnerability of national economies to supply and demand conditions in the international economy had marked effects on national economic performance. The parameter estimates for the instruments suggest that past rates of growth, change in inflation rates and change in unemployment levels were positively related to contemporary macroeconomic outcomes. Higher OECD demand and export prices (indicative of boom periods) were associated with higher rates of growth, lower rates of change in inflation and lower rates of increase in unemployment. Higher OECD import prices (generated by the OPEC oil shocks and the like) were associated with lower rates of growth and higher rates of change in inflation, but with slower growth in unemployment.

16. Details of the robustness tests may be obtained from us. We also have estimated models for inflation and unemployment without differencing the variable (hence we have yearly percentage changes in inflation and unemployment rates). Since without differencing, the time series is not stationary, we have little confidence in the efficiency of these estimates. There are two significant differences between these undifferenced and differenced results. In the former, the effects of the labor organization index are substantially larger and two of the international economic controls change signs: OECD export prices in the inflation model and OECD import prices in the unemployment model.

17. Note that conditional coefficients could be generated for any level of LORG. We have chosen, however, to report only four cases within the range of the sample: those at the top (Sweden) and bottom (Japan) and two intermediate values (Finland, sixth in the sample, and Germany, eleventh). See

Appendix.

18. We also examined the impact of each component of LORG—centralization and density—to determine whether they had different and independent effects on economic performance. The models in Table 1 were reestimated with both the density and centralization variables standing as indicators of labor market organization. In general the patterns of results were very similar to those reported for the summary labor organization index models. The only exception was for the union density model of changes in inflation, where the centralization coefficient was negative rather than the expected positive. Details of these models may be obtained from us.

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PARTY IDENTIFICATION, REALIGNMENT, AND PARTY VOTING: BACK TO THE BASICS

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The argument is presented for defining party identification by the root question, "Generally speaking, do you usually think of yourself as a Republican, a Democrat, an independent, or what?" With this definitional base, the partisan balance between Democrats and Republicans between 1952 and 1980 shows no evidence of realignment outside the South, belying the implications of the Markus–Converse and Fiorina analyses that suggest volatility in response to short-term influences. It also appears that the correlation between party identification and voter choices for president are very constant over time in the South as well as outside the South. Party line voting by party identifiers varies by region and party but did not decrease between 1952 and 1988.

I argue the utility of distinguishing between the overlapping concepts of partisanship and party identification. I do so by presenting some of the consequences of limiting the measurement of party identification to the responses evoked by the classic root question, "Generally speaking, do you usually think of yourself as a Republican, a Democrat, an independent, or what?" In presenting this set of findings as a research note, I will not attempt to add to the literature on the differences between strong identifiers and weak identifiers, nor will I reexamine the many interpretations of the partisan sympathies of "independent leaners."

Even without undertaking such tasks, my mode of presenting data on the historical record of party identification, narrowly defined, calls into question at least a portion of the current conventional wisdom about the nature of party identification and about the responsiveness of party identification to economic and social events in the lives of individual voters. I shall question the conclusion that a national party realignment preceded the election of 1984. I shall also question the conclusion that dealignment has reduced the relevance of party identification for the vote choice. Finally, in pursuing these conclusions I shall question some revisionist arguments concerning the impact of short-term influences on party identification.

Party Identification and Its Operational Measure

My approach to encouraging reconsideration of some of the conventional wisdom about party identification has three major structural components. First, I employ a conceptual definition of Democrats and Republicans that rests entirely on answers to the root question, "Generally speaking, do you usually think of yourself as a Republican, a Democrat, an independent, or what?" People who answer "independent" or "no preference" or "some other party" are not treated as

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Republicans or Democrats, even though they may subsequently admit to being closer to one of the two parties. Moreover, I will not distinguish between strong or weak partisans. Thus my basic measure is restricted to separating party identifiers (Democrats and Republicans) from nonidentifiers.

The reasons for this strict interpretation of the meaning of party identification are more firmly grounded in theory than in data. There is little question that variations in the "strength of partisanship" have reflected variations in the short-term fortunes of the respective parties, and have led to changes in the sheer intensity of partisan enthusiasms. It has also been well documented that the same short-term forces have both attracted and repelled many citizens who, while *not* major party identifiers, have on different occasions seen themselves to be closer to one of the two major party alternatives. However, neither of these considerations speaks directly to the question of individuals responding to the same short-term influences by self-consciously moving *across the boundary* separating identifiers from nonidentifiers. The question is not whether "independent leaners" may, from time to time, be more partisan in their voting or their issue preferences than are weak identifiers. And the question is not whether independent leaners are covert partisans; they are demonstrably and overtly partisan. The question is the stability (and the meaningfulness) of one's self-identification as a Democrat, a Republican, or as something else.

In searching for an answer to this question I "return to the basics" as I reconsider the original treatment of the concept and the measurement procedures reported in *The American Voter* (Campbell, et al. 1960, 121–28). A return to the source places the original measurements in context. It is clear that the effort in *The American Voter* was to build on the concept of group (party) identification, but *also* to create an indicator that would differentiate among degrees of "partisanship" or "partisan coloration." With hindsight, it now seems that the effort to maximize the versatility of an operational measure blurred the clarity of the basic concept of identification with a political party. On the one hand, going beyond the root question to differentiate additional degrees of partisanship muddled the dimensionality of the resulting measure. It introduced intransitivity into a presumed continuum. Most important here, it also created indicators of *partisanship* that were reflective of short-term influences of preferences for issues or for candidates as well, perhaps, as variations in the relatively enduring sense of partisan political self that is the explicit heart of the concept of identification (Brody 1978; Keith et al. 1986; Miller 1991).

The significance of attending to "details" of measurement when analyzing party identification has recently been forcefully argued by Converse and Pierce. They emphasize that there are "two elements which have been absolutely central to the whole notion of party identification: an *extended time horizon* and some engagement of partisan feelings with *self-identity*. . . . These two elements . . . imply . . . that numerous forms of partisan feelings may be experienced by an individual, and reported upon to investigators, which *do not* constitute the possession of a party identification as such" (emphasis added; Converse and Pierce 1987, 143). In this exercise we have set aside the differentiation of independent leaners. We have done so in part because in the original interview sequence the independent leaners clearly deny a "temporally extended self-identity" as Democrat or Republican. It is also true that the follow-up question, "Do you think of yourself as closer to the Republican or Democratic party?" does not attempt to elicit a qualified or limited sense of an "enduring engagement of partisan feelings with self-identity"; the question is asked only in the present tense, and it calls only for a cognitive assessment of current circumstance.[1] The answers may indicate partisanship but they do not reflect a sense of party identification. I agree with Converse and Pierce when they note that there may be no "right" way to measure *partisanship* but "it is of great importance not to treat diverse measures of partisanship as functional equivalents of one another. Partisanship has multiple facets, and keeping clear which facet is being measured, is a basic investigator responsibility."[2]

One result of the common practice of attending to variations in strength of party identification (strong or not so strong), and variations in the partisan sympathies of nonidentifiers (the so-called independent leaners) has been to obscure the relative stability of the basic sense of the political self, elemental party identification. Short-term enthusiasms for a Lyndon Johnson, Democratic dismay with a George McGovern, and Republican distress with Watergate are clearly reflected in abrupt changes in the now traditional *seven point* measure of partisanship; I shall document the very limited impact of such phenomena on answers to the basic identification question, "Generally speaking, do you usually think of yourself as a Republican or a Democrat?" (Brody 1978; Shively 1979; Claggett 1981).[3]

In a related development, attention to variations on the strength dimension of partisanship had focused interest on what has come to be called dealignment. At some point in the reconsideration of party identification, the role of microlevel dealignment as a forerunner to systemic realignment must be taken up anew. Because of the limited goal of this research note I shall not present, nor follow the implications of, data connecting dealignment to realignment. It is enough to be concerned with the historical record of party alignments, e.g., the empirical record of the numerical balance between those who are self-identified Republicans and Democrats. That record will be based on the classic definition of *party identification* rather than on reflections of the broader construct of *partisanship* (Miller 1991).

The Historical Record

The second major structural component of my analysis is the decision to exploit the full 36-year time series array of National Election Studies presidential election study data, 1952–88. Examining the full sweep of the period covering 10 elections provides a historical context essential to the analysis of party realignment, as well as to the simple study of aggregate indicators of stability and change through time.

Subgroup Differences

The third element in my strategy of inquiry is to consider, more or less in tandem, several strands of evidence that are usually presented in isolation, one from the other, in the literature. To this end I shall "disaggregate" the electorate and examine such constituent segment in the presence of all other segments; the parts will sum to the whole, but I will be able to assess the contribution of each part to the whole.

Electoral Participation

My first disaggregation separates voters from nonvoters. I shall note that the party identifications of nonvoters among various subgroups in the electorate differ from the party identifications of voters in the same subgroups. I shall also note that combining the two often obscures patterns that characterize voters alone. This must certainly mean that analyses relating aggregate national distributions of party identification in the total electorate to aggregate national election outcomes have missed the mark insofar as the divergent distributions of *nonvoters* have been permitted to intrude on the interpretation of the electoral divisions among *voters.*

Race

My second disaggregation comes from separating the electorate on the dimension of race. Ideally I would like to examine each of the contributions to the nation's growing ethnic diversity. Because of the limited numbers of Hispanics, Asians, and other minorities in the samples, I trace only the black citizenry over the past four decades. However, separate attention to the partisanship of black citizens is crucial because of the dramatic changes in their contribution to the nation's politics. Even so, the numbers in the national samples are too small to disaggregate blacks into categories other than voters and nonvoters.

Region

The third focus for disaggregation of nonblacks in my historical reconstruction of stability and change in party identification adds the theme of regional differences. At least since Converse's discussion, party realignment in the South has been an acknowledged topic of importance in contemporary political analysis (Converse 1963; Wolfinger and Hagen 1985). Earl and Merle Black added to the work of Converse, Campbell, Beck, Petrocik, Wolfinger, and others and analyzed the continuation of change in the South during the Reagan years (Black and Black 1987). Curiously, however, there was no immediate follow-up to the Blacks' work to ask what the analytic removal of the South did to the remaining national estimates of party identification. I shall ask and answer the question immediately by adding a South/non-South comparison to our comparisons of voters and nonvoters among nonblacks.[4]

Gender

In my "return to basics," as I examine the record of party identification distributions over the past 10 elections, I shall add one more dimension, *gender.* Gender-related differences usually drew comment in the 1950s to explain how widows were responsible for the slightly pro-Republican cast to the female vote of the 1950s. In more recent years, the persistent pro-Democratic, presumably liberal, cast of women's votes (when compared to male votes) has been labeled "The Gender Gap." As an empirical matter, it is real. In all of the recent elections the female vote has been more Democratic than has the male vote, and this contrasts rather sharply with both the 1950s and 1960s. Our last question, therefore is: "Is there now a 'gender gap' among nonblack voters—South or non-South—that constitutes an element of party realignment?"

Still other dimensions of interest to both practical politics and political theory, such as religion and age, could be added to this list. However, it is not necessary to go beyond the set I have selected in order to make the point that a reassessment of the historical record of the nature and role of party identification is needed. Among the dimensions of disaggregation I have specified, the distinction between voters and nonvoters is the most important (Epstein 1985; Wolfinger and Hagen 1985; Kelley 1988).

The Distribution of Party Identification

Somewhat arbitrarily, I first draw attention to evidence related to party realignment and the regional contrast, South and North, depicted among white voters in Table 1. Between the elections of 1952 and 1980, outside the South, neither men nor women voters revealed any significant change in the net balance of their partisan sentiments across the 30-year interval. With gender differences exceeding two percentage points during this period only in 1964 and 1976, this is a remarkable demonstration of stability. For Northern whites, the "steady state" period of relatively unchanging party identification apparently lasted not 12 years—1952 to 1964—but a full 30 years and was finally interrupted only *after* the first Reagan election.[5]

This extraordinary display of persistence in the net party balance among the Northern white voters, who made up between 75% and 80% of all white voters over the past four decades, provides a striking implicit commentary on the literature on the stability of party identification and party alignment. It does not, of course, necessarily negate analyses of short-term fluctuations between the quadrennial readings, although both Green and Palmquist (1990) and Abramson and Ostrom (1991) have recently spoken to

Table 1. Partisan Balance of Party Identification Within Selected Groups of Voters and Nonvoters,[a] 1952–88

	Non-South		South		Nation	
Election Year	Men	Women	Men	Women	Blacks	All
Voters[b]						
1952	4	3	57	54	52	15
1956	3	2	57	43	36	12
1960	1	3	58	43	30	15
1964	10	6	39	39	75	32
1968	3	4	28	39	87	22
1972	1	0	21	23	68	13
1976	3	-3	25	14	70	12
1980	3	5	3	21	80	15
1984	-10	-3	5	19	73	5
1988	-9	-7	-3	23	80	4
% Distribution						
1952	41	38	8	9	4	100
1988	30	36	11	13	10	100
Nonvoters						
1952	24	22	49	50	43	39
1956	14	-4	53	46	41	24
1960	10	20	44	59	30	34
1964	31	47	49	35	69	48
1968	8	18	21	27	71	32
1972	12	16	19	29	55	24
1976	-4	12	16	48	52	24
1980	0	24	28	18	60	24
1984	7	8	20	13	52	17
1988	-2	-1	8	26	43	14
% Distribution						
1952	19	27	9	24	20	100
1988	21	29	14	20	17	100

[a] Each entry is the proportion of Democratic identifiers (strong plus weak) minus (−) the proportion of Republicans identified (strong plus weak). A negative sign indicates a Republican plurality.

[b] Voters defined as "validated voters" in 1964, 1976, 1980, 1984, 1988; in all other years the definition is provided by the respondents' self-reports in the postelection interview.

this point. However, the evidence of pervasive, long-term, aggregate stability outside the South is so dramatic that it would seem to call for a reexamination of many conclusions about the origins of change in party identification. It at least calls into question analyses that have, for example, used changes in *national* economic indicators to explain *national* changes in party identification, when electorally relevant change in party identification apparently did not take place outside the South.

It raises more direct questions about the implications of the thesis that party identification changes incrementally as the consequence of prior voting behavior. From 1952 to 1980, according to National Election Studies data, the Northern vote division among white voters averaged *57% Republican.* Despite very large Republican pluralities in virtually every year except 1964, the Republican share of party identifiers did not increase as a simple extrapolation of the work of Markus and Converse (1979) might have suggested.

The same evidence of stability in the partisan balance of party identification calls attention to the absence of any cumulative impact of a series of "running tallies" that should have produced a Republican increment between 1952 and 1980 (Fiorina 1981). The aggregate stabilities may, of course, conceal compensatory changes that have offset a drift away from the Democrats and into the Republicans' camp. No reasons for, or evidence of, such compensatory, pro-Democratic changes come immediately to mind. Consequently, it seems fair to conclude that the evidence of aggregate stability among nonblack voters outside the South should prompt further study of the dynamics of microlevel change in party identification.[6]

Realignment in the South

Equally clear evidence of the mutability of partisan loyalties is provided by Table 1 and its description of change among Southern white voters in general, and white Southern males in particular, across the same time span. Apparently the beginning of the end of single-party dominance among Southern white male voters started shortly after the Kennedy election of 1960. By the time of the first Reagan presidency, 20 years later, a virtual 80–20 division favoring the Democratic party had been replaced by near parity for the Republican party. This would seem to be evidence of a classic version of the realignment of partisanship. It was a realignment of massive proportions, involving a Democratic-to-Republican switch of at least 3 out of every 10 Southern nonblack male voters. It was apparently a secular realignment as defined by V. O. Key, Jr. (1959) and introduced in the current discussion by Converse (1976).

Further analysis is needed to determine the relative importance of contributions from conversion, mobilization, and cohort replacement among Southern white voters over the 36 years included in Table 1, but the net effect is unmistakable. While conditions outside the South did not provoke any net change in the party alignment of nonblack voters between 1952 and 1980, changes within the South produced a virtual revolution. A closer examination of both sets of circumstance should tell us more about party identifications. Earl and Merle Black (1987) have suggested major themes to be explored in the analysis of the South. Outside the South suggestions of cohort replacement in the changing composition of the electorate offer a promising first line of inquiry.

The Gender Gap

The possible unitary nature of regional factors capable of producing such party realignment among Southern white male voters is initially reflected in the parallel shift among nonblack Southern female voters across the seven elections between 1952 and 1976. A complicating anomaly then appears. Changes in party identification among Southern female voters after 1976 do not match the pattern of any other set of nonblack voters. This is striking because the other three groupings all reveal a shift to small Republican pluralities at the conclusion of the Reagan era in 1988, while white Southern women continuously exhibit a set of clearly pro-Democratic preferences throughout the 1980s. Moreover, their Democratic plurality in 1988 matches the figures from 1972 and results from a clear increase, not a decrease, in relative Democratic strength after the election of 1976. Why this should be so is not obvious.

The importance of explaining the male–female differences among nonblack Southern voters is accentuated by the realization that those differences in the 1980s are primarily responsible for the much discussed "gender gap" for those years (Baxter and Lansing 1983; Frankovic 1982). Even without "understanding" these gender differences, it seems possible that the appearance of the gender gap in the Reagan years was not as much a function of a liberal, pro-Democratic growth in the partisan sentiments of women as a function of the sharply conservative pro-Republican move among men (Wirls 1986). The Republicans have not had a new problem with women; the Democrats have had a continuing problem among men. And once regional data are sorted out, the specification of the gender problem is largely confined to the South.

Black Voters

Turning away from region and gender differences within white voters to consider the partisan sympathies of the black citizenry, the separation of black voters and black nonvoters from the remainder of the electorate highlights the recent contributions of black voters to partisan national elections. The apparent impact of the Kennedy-Johnson era was more dramatic among black voters than nonvoters. And, given their mobilization beginning in the mid-sixties, black voters across the nation were ultimately only slightly fewer in number than were Southern, white male voters. As a consequence, the fact that black voters across the nation almost tripled their margin of support for the Democratic party between 1960 and 1968 by itself more than offset, in sheer numbers, the 50% decline in the Democratic plurality among Southern white males.

The countervailing moves within these two politically significant segments of the electorate underline the hazards of drawing conclusions based on national aggregations. National totals did not suggest any net change in the national balance of party identifications between 1956 and 1980. This is true, in general, because the precipitously real pro-Republican shift in the white (male) South was counterbalanced by the large growth in Democratic pluralities among newly mobilized black citizens.[7]

Voters and Nonvoters

A suggestion that variations in the nature and meaning of party identification may be associated with variations in political awareness and involvement is provided by the comparison of voters with nonvoters among blacks. Although changes within each group were similar between 1960 and 1964, sharp differences appear after 1968. Those differences are accentuated by the contrary movements between 1984 and 1988. It was black *nonvoters*, but not black voters, who contributed to the aggregate evidence suggestive of party realignment during the Reagan years.[8]

More generally, my analytic separation of voters from nonvoters exposes relatively well-ordered evidence of stability among Northern white voters, stability that contrasts with greater volatility among Northern nonvoters. Both in the unevenness of patterns of change across time and in the variability of gender differences, party identification among nonvoters seems less a matter of stable, long-term predispositions and more a matter of responsiveness to short-term, election-by-election fluctuation of circumstance than is true for voters. A similar contrast appears both among black citizens and among Southern whites, although it is less striking on first inspection because of the pervasive patterns of change within these groups. While pondering the reason for these differences between voters and nonvoters, it should also be recognized that the differences may contain the basis for reconciling other scholars' conclusions about the apparent general responsiveness of party identification to short-term influences with the new evidence of interelection stability among Northern white voters.

As a final comment on Table 1, it may be noted that the particular disaggregations that are so revealing of different patterns of stability and change between 1952 and 1980 do not serve particularly well to cast new light on the more recent changes in the distributions of party identifications that reflect a limited national realignment during the Reagan era. The 1980–84 and 1984–88 changes in party balance were primarily a Northern, white phenomenon and were not apparent at all among black voters or Southern white women voters. A detailed analysis of the realignment of the Reagan years, developed elsewhere, indicates two quite distinct stages of change in which Reagan's personal leadership and then his partisan ideology first moved the younger, less politicized voters and then the older, more politicized into the Republican party. In neither stage of the realignment after 1980 were race, region, or gender as relevant as age, ideology, and political involvement (Miller 1990). The narrowest point to be drawn from my introductory analysis is, quite simply, that systematic inspection of disaggregated time series data may have been a good starting point for questioning much conventional wisdom about party realignment and the stability of party identification, but it does not provide answers to many of the questions that it raises or that I have posed. The broader points of theoretical interest in the analysis are the many implications for shaping future inquiry into the nature of party identification and into the conditions that facilitate its stability, or provoke change.

Party Identification and the Vote

Just as conventional wisdom about the stability of Democratic and Republican party identifications has encouraged some dubious interpretations of changing *distributions* in party identification, questions have also been raised concerning the meaningfulness of party identification as a *determinant* of the vote during the 1970s and 1980s. It seems reasonable to presume that two notable deviations in the correlation of party identification with preferences for presidential candidates, first in 1964 and then again in 1972, have been the remembered evidence for presuming a generally diminished importance for party following the elections of the 1950s (Miller, et al. 1976). A systematic examination of the simple national bivariate relationships between party identification and vote choice over time, presented in Table 2, documents both episodes of declining correlation; it also documents the atypicality of the 1964 and 1972 elections.

The left hand column of Table 2 reveals only two elections in which the unstan-

Table 2. Correlations of Party Identification with the Presidential Vote Choice, 1952–88

Election Year	Bivariate	Partial[a]
1952	.69	.64
1956	.67	.65
1960	.68	.60
1964	.57	.48
1968	.69	.55
1972	.52	.43
1976	.69	.64
1980	.73	.63
1984	.74	.65
1988	.75	.68
Mean	.67	.60

[a] Partial correlation with controls on race, education, gender, religion, income, and union membership.

dardized bivariate correlation falls well below the 10-election average of .67. They were the Johnson-Goldwater contest, in which Republicans voted against their conservative senator from the desert, and the Democrats' disastrous experiment with the radical liberal McGovern challenge to an immensely popular Republican incumbent, Richard Nixon. After having risen steadily from the 1972 low, the party identification/vote choice correlation was up to postwar highs by 1984 and 1988. There is no indication from any recent election that party identification is less relevant to the vote decision in the 1980s than it was three decades earlier.

As a further test of the constancy of correlations with the vote, the second column of Table 2 presents the *partial* correlations of party identifications and vote choice after the simultaneous imposition of controls on race, gender, religion, education, income, and union membership. The dual message of the partial correlations is clear: (1) year after year very little of the party–vote correlation can be considered the spurious consequence of their sharing these common antecedents; and (2) the passage of time has seen no diminution in the total effect of party identification on vote choice.

The causal role of party identification in the shaping of the attitudes and perceptions that are the immediate or proximate causes of a voter's preference for one candidate over the other had been a continuous topic of inquiry among students of electoral behavior. Evidence that the causal role of party identification as an antecedent to the vote is largely indirect through its influence on policy preferences and appraisals of presidential performance and candidate traits is provided by the Shanks and Miller (1990) analyses of the Reagan elections. Such inquiries can continue to be motivated, at least in part, by the knowledge that over time the *correlational* evidence is stronger, not weaker, for assigning a major explanatory/causal role for party identification in the context of presidential elections (Beck 1977, 1982; Fiorina 1977, 1981; Markus and Converse 1979; Page and Jones 1979; Petrocik 1981; Wattenberg 1984; Whiteley 1988; Jacoby 1988).

Party Voting

The more general conclusion that there has been no across-time decrease in the extent to which the national presidential vote is a party vote is neither challenged nor further illuminated by our disaggregation of voters by gender, region, or race. Year in and year out, women have been no more likely than men to cast a party vote *nor* to defect and cross party lines to vote for a president. After 1960, the black vote was as unwaveringly Democratic as were black voters' partisan loyalties. And within regional comparisons, as with the others, party voting in the 1980s was every bit as common—or uncommon—as it had been in the 1950s. Voting in line with one's party in 1984 and 1988 was as common as it had been in 1952 and 1956.

This conclusion holds for partisans on both sides of the aisle. Republicans in the South have an almost perfect record; they have reported voting for Republican candidates an average of at least 95% of the time from the days of Eisenhower and Nixon to the era of Reagan and Bush. Outside the South the Republican vote has been slightly more variable, but Northern Republicans have reported more than 90% support for their party's presidential candidates. The most noticeable, if still minor, occasions for defections came in 1964 and 1976, but the Republican figures for 1984 and 1988, both South and non-South, and among both men and women, were fully equal to their reported party votes in 1952 and 1956.

The record of Democratic identifiers at the polls is a persistent record of substantially *less* party support than given by Republicans, and it is somewhat more varied than the record of Republicans. Nevertheless, the Democratic patterns of regional party voting do not appear to have changed at all over three decades. Democrats, regardless of either region or sex, were just as faithful—or unfaithful—to party in their 1984 and 1988 votes as in earlier decades. There is no evidence pertaining to either party in either region of any difference in the level of party voting among party identifiers that would distinguish the 1980s from the 1950s.

There are real world consequences of this observation that extend well beyond what it says to political scientists about the more or less enduring nature and role of party identification. The data presented in Table 1, and my earlier discussion, indicate that Democrats quite apparently declined in numerical strength in the South between 1960 and 1980, and across the entire nation during the Reagan era, 1980–88. This happened without the emergence of greater loyalty or more faithful voting performance from those who *remained* as Democrats. At the same time, Republicans increased their numbers among party identifiers without suffering any dilution of party loyalty at the polls. Even in the changing South, the party identification/vote choice relationship has been stable for more than three decades for *both* Democratic and Republican identifiers. Thus it would appear that there as elsewhere typically less-than-faithful Democrats have converted to become typically faithful Republicans. Because Republican identifiers are substantially more faithful party voters than are Democratic identifiers, both elements in the exchange argue a greater increase in the election day strength of Republicans than implied by the simple distributional shifts in party identifications. The potentially good news for the Republican party rests on the validity of my deductions based on a return to the basics. Even though a massive regional realignment, followed by a limited national realignment, has cost the Democrats their one-time advantage in the distribution of party identification, the implications of party identification for the presidential vote have not changed materially with the passage of time and the change of political circumstance. And in 1984 and 1988 the Republican ticket benefited doubly from the leveling of the partisan playing field.

Notes

I am indebted to support extended to Regents Professors by Arizona State University. Data processing was done by Tao Wu; manuscript preparation was done by Linda Coddington and Julie Verrill. The data were created by the Center for Political Studies of the Institute for Social Research at the University of Michigan with grants from the National Science Foundation, and made available by the Inter-University Consortium for Political and Social Research. I also thank Paul Beck, Morris Fiorina, John Geer, Ruth Jones, Kim Kahn, Donald Kinder, Patrick Kenney, and Raymond Wolfinger for critical comments and advice.

1. The emphasis on an extended time horizon is properly the center of the recent Abramson and Ostrom critique of "Macropartisanship" by

MacKuen, Erikson, and Stimson (1989).

2. It should be noted that the authors of *The American Voter* have, both early and late, contributed to the conventional wisdom against which this note is directed. For example, in *The Dynamics of Party Support* (Converse 1976) weak identifiers were often combined with independent leaners in the measurement of "strength"; and in the *American National Election Studies Data Sourcebook* (Miller and Traugott 1989) summary measures of the partisan balance of party preference combine partisan leaners with party identifiers, as in Table 2.32, pp. 103–104. It might also be noted, however, that the definition of party identification groups displayed in tables in *The American Voter* usually followed the orthodoxy being promoted in this note, and the *Data Sourcebook* treats party identification as separate and distinct from party preference, as in Table 2.34, pp. 105–106.

3. In this regard my conclusion is on all fours with the analysis of Abramson and Ostrom who demonstrate that the temporal stability of party identifications can be missed—and often has been missed—when Gallup-like questions that emphasize the current moment ("In politics as of today . . .") are used as indicators of party identification (Abramson and Ostrom, 1991).

4. It should be noted that for simplicity of expression I shall, at times, refer to the non-South as the North, even though I mean Westerners, Mid-Westerners as well as "Northerners"; and, taking somewhat greater liberties, I shall refer to nonblacks as whites even though this group includes Hispanics and other ethnic minorities in very small numbers.

5. My earlier comment on measurement comes into play here. If one focuses on "strength of partisanship" and takes variations in the proportions of "strong" identifiers *and* the proportions with no partisan preference into account, one sees the aggregate evidence that prompted discussions of dealignment. These variations are *not* reflected in root self-identification for nonblack voters outside the South. For an elaboration of this discussion, see Miller 1990.

6. Further work on the responsiveness of party identification to short-terms forces should also take into account the analyses by Donald P. Green and Bradley Palmquist (1990). Their analyses suggest the importance of taking measurement error into account when looking for evidence of partisan instability in nonrecursive models.

7. I am indebted to David Leege for the observation that, given the dispersal of black voters throughout the nation, their mobilization did not redress the potential shift in electoral votes resulting from the regional concentration of Democratic losses and Republican gains in the Southern states.

8. One consequence of our separation of voters and nonvoters should be a reinvigoration of interest in citizen turnout. It is quite possible, for example, that some portion of the apparent pro-Republican swing among Southern, nonblack, male voters in the 1980s may have been caused by declining turnout among Southern, nonblack *Democratic* males rather than by pro-Republican conversions within a constant, unchanging population of voters.

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CONTROVERSIES

SPECIFYING A MODEL OF STATE POLICY INNOVATION

How should policy innovations undertaken by states be modeled? Frances Stokes Berry and William D. Berry presented an event history analysis of the determinants of lottery adoptions by state governments in the June 1990 issue of this Review. *Howard Frant argues that the way Berry and Berry tested for interaction among variables is invalid on the grounds that what they take to be empirical results are only artifacts of the model specification. In response, the Berrys elaborate their original model and add alternative specifications.*

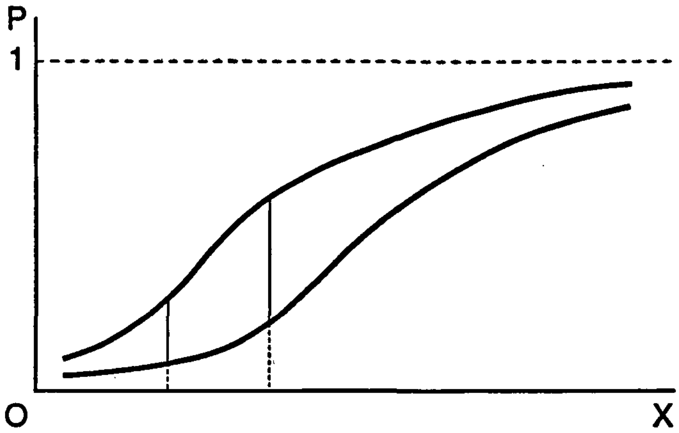
Frances Stokes Berry and William D. Berry (1990) demonstrate the use of event history analysis to measure the determinants of adoption of lotteries by state governments. The latter part of their article, however, reveals a methodological misconception: they present as empirical results what are in reality artifacts of the way the model is specified. Since the stated motivation for the analysis is to illustrate a methodology, this misconception needs to be cleared up.

Berry and Berry use a probit model and time series data to test hypotheses about the effects of a number of independent variables on a state's probability of adopting a lottery in a given year. Once coefficients of the independent variables have been estimated, a predicted probability can be calculated for any given values of the independent variables. So far, so good.

The problem arises when Berry and Berry attempt to test a series of hypotheses about the interactions between independent variables: that the effect of religious fundamentalism will be greater when a state's fiscal health is poor, that the effect of personal income will be greater in an election year, and so on. The authors "test" these hypotheses by calculating the predicted probability of lottery adoption given various values of the independent variables. They find, for example, that the effect associated with an election year is greater (in absolute terms) when the state is in poor fiscal health than when it is in good fiscal health. This sounds like a *conclusion* (and the authors treat it as such); but actually, it is an *assumption* of the model.

Figure 1 shows a generic probit estimate of the effect of a continuous variable with and without a dummy variable. (I use one continuous and one dummy variable purely for graphical simplicity.) Since both lines approach zero and one asymptotically, they get arbitrarily close together as the predicted probabilities get sufficiently small or large. The distance between the two lines is greatest in the intermediate range, when the predicted probabilities are around .5. This says that the effect of the dummy variable on the predicted probability is small when the a priori probability is very high or very low and large when the event is "on the margin," that is, when it might go either way with almost equal probability. This is a plausible assumption, but it is only an assumption. When we employ a probit (or logit) model, this assumption is built in.[1]

Figure 1. Probit Estimates of the Effect of a Continuous Variable with and without a Dummy Variable



Since in Berry and Berry's data predicted probabilities are quite low, anything that increases the probabilities will increase the (absolute) distance between the lines. This accounts for *all* of their supposed interaction effects.

Indeed, we can use this knowledge of the model to generate a large number of completely atheoretical predictions, all of which will be "confirmed" by calculation of estimated probabilities. For example, Berry and Berry find that per capita income has a positive effect on probability of adoption. We can therefore say confidently that the calculated (negative) effect of religion will be greater in states with greater per capita income, even though there is no theoretical reason to expect this. Table 1 (which uses data from Berry and Berry's Tables 1 and 2) shows that this is in fact the case.

To drive home the point that these interaction effects are assumptions rather than conclusions, suppose that Berry and Berry had, instead of a probit model, chosen the somewhat less sophisticated alternative of estimating probabilities by ordinary least squares. They could then have calculated estimated probabilities for given values of the independent variables, just as they did with the probit model. Inspection of Figure 2 shows that had they done this, they would have concluded that the effects of a given variable did *not* vary with the level of other variables. The point here is not that one model is better or worse than the other (though it

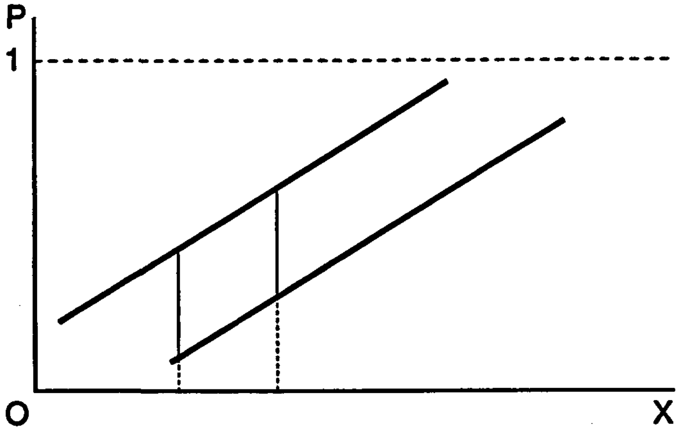
Table 1. The Effect of Religion on State Adoption of Lotteries Controlling for Per Capita Income

Income	Religion[a]	Predicted Probability
High (135.9)	Low	.55
	Medium	.32
	High	.15
Moderate (89.5)	Low	.18
	Medium	.06
	High	.02
Low (41.3)	Low	.02
	Medium	.004
	High	.001

Note: Values of other variables: election year (ELECT1 = 1, ELECT2 = 0); two neighbors; fiscal health poor (-.2).

[a] Low = .7, medium = 17.6, high = 34.4.

Figure 2. Ordinary Least Squares Estimates of the Effect of a Continuous Variable with and without a Dummy Variable



may be) but that whether we find interaction effects by this method does not depend at all on the data. Rather, it is predetermined by the choice of model.

If Berry and Berry wish to test for interaction effects, they can do this in a probit model in the same way as one would in ordinary least squares, that is, by including interaction terms (e.g., ELECT1 • FISCAL) in the model. The degrees of freedom in their data appear more than adequate to permit this. Until that is done, no conclusions can be drawn about interactions.

HOWARD FRANT

University of Arizona

In our June 1990 article, we test an explanation for the adoption of lotteries by states with event history analysis, using pooled cross-sectional time series data and probit for estimation. Part of this explanation involves Mohr's (1969) hypothesis that the motivation to innovate interacts with both the strength of obstacles and the amount of resources available in influencing the probability that a state government will adopt a lottery. Frant offers two basic criticisms: (1) that our empirical results about interaction are "artifacts of the way the model is specified" and (2) that a more appropriate way to specify interaction would be with multiplicative terms in the probit model. We will argue that neither of these positions is justified. In the course of evaluating Frant's criticisms, we will also present an alternative specification—a linear probability model with multiplicative terms—that generates substantive conclusions similar to those presented in our article. We now realize that our justification for our treatment of interaction was insufficient; for in an effort to keep a long paper from being even longer, we relegated the justification primarily to the notes. We are indebted to Frant for raising the interaction issue and prompting us to elaborate our argument.

At the outset, we should note that even if Frant's contentions were correct, our analysis would still achieve its central theoretical and methodological objectives: (1) to suggest that neither internal determinants nor regional diffusion explanations of state policy innovation are plausible in isolation, (2) to introduce a pooled cross-sectional time series approach to state innovation research that would allow for a test of a theory reflecting both internal and regional influences on adoption probability, and (3) to illustrate the potential of event history analysis for testing explanations of rare political events, such as policy adoptions. Moreover, even if, as Frant claims, our probit models do not reflect "interaction," they still lend support to substantively and theoretically meaningful propositions about the factors determining the conditions under which states adopt lotteries.

Are Our Results an Artifact?

Frant's first argument is that our empirical results about interaction are an "artifact" of our methodology. The example offered is our empirical evidence (in Table 2 of our article) that the effect of election proximity on the probability of an adoption increases as the fiscal health of a state's government decreases. Frant claims that this result "sounds like a *conclusion* (and the authors treat it as such); but actually, it is an *assumption* of the model." It is true that interaction among variables in their effects on the probability of adoption is an assumption of our probit model. But all multivariate models make *assumptions* about the functional form of the effects of independent variables; yet substantive conclusions are drawn from empirical analysis of these models. Indeed, an interactive regression model with a multiplicative term (e.g., $Y = \alpha + \beta_1X_1 + \beta_2X_2 + \beta_3X_1X_2$)—similar to the form that Frant proposes we use—also *assumes* interaction among the independent variables (provided $\beta_3 \neq 0$). And when support is claimed for hypothesized interaction based on coefficient estimates from this multiplicative model, it is meant that the empirical evidence is *consistent* with the assumption of interaction and that if that assumption is indeed correct (and the other regression assumptions are met), the coefficient estimates of the model can be used to estimate the effect of one independent variable on the dependent variable at any level of the other independent variable. When we claim support for our hypotheses of interaction, we employ a similar strategy: we assume that interaction takes place and conclude that the empirical evidence is consistent with that assumption.

Furthermore, we do not believe that our empirical evidence consistent with Mohr's (1969) interaction hypotheses is an artifact of our model specification, as Frant contends. We acknowledge that our "reliance on the probit specification for our model makes it so that our hypotheses predicting interaction are not fully distinct from hypotheses about coefficients for individual independent variables. . . . As long as the coefficients for the independent variables are nonzero, some interaction among the independent variables in influencing the probability of adoption is guaranteed" (Berry and Berry 1990, 414). Therefore, we certainly cannot claim to have tested a hypothesis of "interaction" against a null hypothesis of "no interaction." However, empirical analysis could have generated results that would necessitate rejection of the interaction hypotheses. First, if the coefficient estimates for our model proved to be weak, the interaction effects observed would not be substantively appreciable. Second, while the probit model guarantees the presence of some observed interaction among independent variables in influencing the probability of adoption, the specific nature of the interaction will not necessarily be consistent with the hypotheses introduced. Whereas Mohr's theory leads us to predict that the positive effect of the number of previously adopting neighboring states (NEIGHBORS) on the probability of adoption is greater in an election year than in a nonelection year, it is possible (with different "observed worlds") to generate probit maximum likelihood estimates implying that (1) the positive effect of NEIGHBORS is greater in a *non*election year than in an election year, (2) the *negative* effect of NEIGHBORS is greater in an election year, or (3) the negative effect is greater in a *non*election year. Thus, we reject the contention that the empirical support for our interaction hypotheses is a mere artifact of our probit methodology.

The Appropriateness of Multiplicative Terms

Frant's second contention is that to test our hypotheses of interaction properly, we should include multiplicative terms in our probit equation. While this would be sound advice in many probit applications, in our model the introduction of multiplicative terms would result in misspecification. To clarify why this is so, we must take a closer look at the probit specification. In the most typical formulation of probit, an unmeasured continuous interval-level dependent variable (to be denoted Z) is assumed to be a linear additive function of a set of independent variables (Aldrich and Nelson 1984), as in

$$Z = \beta_0 + \beta_1 X_1 + \beta_2 X_2 + \ldots + \beta_k X_k \qquad (1)$$

In our lottery analysis, this unmeasured Z might be interpreted as a state's "inclination" or "propensity" (to use Frant's terminology) to adopt a lottery. The probit model assumes that Z is unbounded; it can take on values as small or as large as the values of the independent variables imply. The functional form of equation 1 makes it clear that the probit model assumes no interaction among independent variables in influencing the unbounded dependent variable Z. If interaction among independent variables in influencing Z is hypothesized, the model must be modified; and at least with some forms of interaction, multiplicative terms can be used to specify the interaction. But in probit, one assumes that there is an *observed* ordinal indicator of Z—in many applications, one that is dichotomous (scored either zero or one)—and that in the dichotomous case the independent variables interact in influencing the *probability* that a case will have a score of one on the observed dependent variable.

Consequently, whether multiplicative terms should be used in our probit model hinges on whether we are predicting interaction among the independent variables in influencing (1) the unbounded (unmeasured) variable, the *inclination* to adopt, or (2) the *probability* of adoption, a variable constrained to the range between zero and one. In the former case, multiplicative terms may be appropriate; in the latter case, they would misspecify the hypothesis. Mohr (1969) does not fully clarify which of these forms of interaction we should expect. But we believe the latter expectation is more reasonable. As an example, consider Mohr's hypothesis that the strength of obstacles to innovation has a strong effect on the probability of adoption when motivation to adopt is high and a weaker effect as motivation to adopt decreases. Would this hypothesis be reasonable if the dependent variable were the unbounded *inclination* to adopt a policy? To see why it would not, we can compare two hypothetical contexts, one characterized by very low motivation to innovate and many obstacles, the other by equally low motivation but few obstacles. The hypothesis predicts that the *inclination* to innovate should be almost the same in these two contexts. But when the dependent variable is totally unbounded, it makes sense that even if motivation to innovate were fixed at a very low level, as the context changes from one free of obstacles to one loaded with obstacles, the *inclination* to innovate should decrease substantially.

We contend that this interaction

hypothesis is much sounder when it is cast in terms of the *probability* of innovation, so that the dependent variable is restricted to the range between zero and one. In this case, when motivation to innovate is fixed at a low level, increasing obstacles from very low to very high is unlikely to change the probability of innovation appreciably; while *inclination* should decrease substantially, the *probability*—restricted to be above zero—would have no room to fall. Thus, we believe that for this hypothesis, as well as for the rest of the interaction hypotheses in our article, the expectation of interaction hinges to a large degree on the restricted range of the dependent variable, the probability of innovation. We are content with the assumption that the determinants of lottery adoption we include in our model are *additive* in their effects on the *inclination* to adopt. As a result, multiplicative terms in the probit equation would misspecify the model.

However, we might ask whether it is appropriate to devote attention to relationships between the independent variables and the *probability* of adoption when, in the typical probit formulation, a dichotomous dependent variable would be conceived as an indicator of a continuous unmeasured concept. For some social science probit applications, theoretical attention is rightly focused on relationships involving the unmeasured dependent variable. For example, when the observed dependent variable is a multiple-category survey response tapping the extent of agreement with an issue position, the measured dependent variable is best conceived as an instrument to learn about the underlying continuous attitude it is designed to measure. In such a case, if the independent variables are additive in their effects on the continuous dependent variable and interact only in their effects on the probability that a respondent will give a particular response (e.g., strongly agree) to the question, the interaction is not theoretically meaningful. But in our application of probit, despite the fact that one may conceptualize the continuous variable, the *inclination* to adopt, *adopt* and *not adopt* are not two categories designed to be used instrumentally to learn about the inclination to adopt; the *probability* of the event *adopt* is of direct interest. In the study of public policy innovation, from both theoretical and substantive perspectives, two contexts that give rise to a .01 *probability* of adoption are virtually identical. It is of little additional practical or theoretical value to learn that the *inclination* to adopt is substantially greater in one context than another.

An Alternative Specification: A Linear Probability Model

Because our observed dependent variable is dichotomous, a linear probability model would be an acceptable alternative to probit. In the linear probability model, a dichotomous dependent variable, D (scored one if an event occurs, and zero otherwise), is assumed to be a function of a set of independent variables and a disturbance term, as in the equation

$$D = \beta_0 + \beta_1 X_1 + \beta_2 X_2 + \ldots + \beta_k X_k + \epsilon.$$

In this model, the independent variables are presumed to have linear and additive effects on the probability that the event will occur (Aldrich and Nelson 1984). However, to specify interactive effects of independent variables on the probability of event occurrence, one may include the same kind of multiplicative terms used in an interactive regression model.

Because our theory of lottery adoption specifies interaction among so many combinations of variables, modeling the theory with a linear probability model would require a huge number of distinct multiplicative terms. For example, just specifying the proposition that the effect of religious fundamentalism on the probability of a lottery adoption depends on the proximity of gubernatorial elections and the fiscal health of a state's government (our hypothesis 6) would require five multiplicative terms: RELIGION • ELECT1, RELIGION • ELECT2, RELIGION • FISCAL, RELIGION • FISCAL • ELECT1, and RELIGION • FISCAL • ELECT2. And if all the interactive relationships we predict were specified, literally dozens of multiplicative terms would be required. Interactive models with both "individual" and multiplicative terms are generally characterized by high multicollinearity (Althauser 1971), and a model with dozens of multiplicative terms would be so crippled by multicollinearity as to be useless for empirical research.

To make a linear probability model of state lottery adoptions feasible, we would need to simplify greatly the form of interaction assumed. One possibility is expressed in the following equation:

$$\begin{aligned}\text{ADOPT}_{i,t} &= b_0 + b_1\text{INCOME}_{i,t-1}\\ &\quad + b_2\text{RELIGION}_{i,t-1} + b_3\text{NEIGHBORS}_{i,t}\\ &\quad + b_4\text{MOTIVE}_{i,t}\\ &\quad + b_5\text{MOTIVE}_{i,t} \bullet \text{INCOME}_{i,t-1} \qquad (2)\\ &\quad + b_6\text{MOTIVE}_{i,t} \bullet \text{RELIGION}_{i,t-1}\\ &\quad + b_7\text{MOTIVE}_{i,t} \bullet \text{NEIGHBORS}_{i,t} + \epsilon_{i,t},\end{aligned}$$

where INCOME, RELIGION, and NEIGHBORS are as defined in our article (pp. 404–5) and $\text{MOTIVE}_{i,t}$ is a dichotomous variable—reflecting the motivation to adopt a lottery—that equals one when it is an election year *and* state government fiscal health is low (in particular, below the median value across the 901 cases observed), and zero otherwise. Our theory would predict that b_2 and b_6 are negative, while the rest of the coefficients are positive. Thus, the effects of religious fundamentalism, neighboring state lottery adoptions, and state personal income are assumed to be stronger when the motivation to adopt is high than when it is low.

Even this greatly simplified linear probability model is characterized by extremely high multicollinearity. The two major sources are MOTIVE and MOTIVE • INCOME, both of which when regressed on the remaining independent variables in equation 2 yield an R-squared greater than .93. Given the multicollinearity, one strategy for estimation is to (1) delete the INCOME terms from the model (regrettably introducing specification error) and (2) restrict plausibly the value of the coefficient for MOTIVE (on the "restriction" approach, see Gujarati 1988, 302–3; Hanushek and Jackson 1977, 92). In particular, we estimate the revised model using ordinary least squares regression and the restriction that $b_4 = .03$; this imposes the assumption that in situations where RELIGION = NEIGHBORS = 0, the probability of adoption is .03 higher when MOTIVE is high than when it is low.[2] The estimates obtained are as follows (where *t*-ratios are reported in parentheses):

$$\begin{aligned}\widehat{\text{ADOPT}}_{i,t} &= \underset{(.295)}{.028} - \underset{(-2.13)}{.00088}\ \text{RELIGION}_{i,t-1}\\ &\quad + \underset{(4.01)}{.030}\ \text{NEIGHBORS}_{i,t} + .03\ \text{MOTIVE}_{i,t}\\ &\quad - \underset{(-1.08)}{.00075}\ \text{MOTIVE}_{i,t} \bullet \text{RELIGION}_{i,t-1}\\ &\quad + \underset{(2.92)}{.049}\ \text{MOTIVE}_{i,t}\quad \text{NEIGHBORS}_{i,t} \qquad (3)\end{aligned}$$

All coefficient estimates for the model take the predicted sign and have sizable magnitudes.[3] Therefore, the linear probability model yields substantive conclusions similar to those derived from probit analysis, despite the need to combine information about the proximity of elections and fiscal health into a single variable, MOTIVE, and dichotomize it at the median, thereby introducing substantial measurement error. Both the probit and linear probability models suggest that the effects of resource availability and obstacles to innovation on the probability of a lottery adoption are greater when the motivation to innovate is high than when it is low.

Ideally, we could perform an F-test of the null hypothesis that $b_6 = b_7 = 0$ to test whether an interactive specification yields more "explanatory power" than the additive specification

$$\text{ADOPT}_{i,t} = a_0 + a_1\text{RELIGION}_{i,t-1} + a_2\text{NEIGHBORS}_{i,t} + a_3\text{MOTIVE}_{i,t} + \epsilon_{i,t}. \quad (4)$$

But the restriction we had to impose on the coefficients of equation 2 make such a test impossible. The problem is that equations 2 and 4 are not "nested" (Judge et al. 1980, 436), and statisticians have failed to derive satisfactory statistical tests for comparing nonnested models. Even a crude comparison of the two models based on R-squared values would be inappropriate, since interpreting R-squared should be avoided with models having dichotomous dependent variables (Aldrich and Nelson 1984, 14–15).

Conclusion: What's the Best Specification?

Since we hypothesize that the interaction among independent variables is in influencing a state's *probability* of a lottery adoption (rather than its unbounded *propensity* to adopt), a probit specification without multiplicative terms or a linear probability model with multiplicative terms are the best choices. Mohr claims that the motivation to innovate interacts with both the strength of obstacles and the amount of resources in influencing the probability of adoption. A weakness of the probit specification is its assumption of interaction among more combinations of variables than Mohr predicts. The linear probability model has just the opposite problem. Specifying all the interactions that Mohr's (1969) theory leads us to predict would require so large a number of multiplicative terms as to render the model useless for empirical analysis because of extreme multicollinearity. Moreover, the necessity to simplify the model by abandoning a specification having separate roles for election proximity and fiscal health in favor of a single motivation to innovate (MOTIVE) variable is a serious weakness of our linear probability model. A final drawback is its difficulty with negative predicted probabilities.[4] Consequently, on balance, we continue to prefer a probit specification without multiplicative terms to a linear probability model with them. In contrast, a probit model including multiplicative terms would unnecessarily misspecify our theory.

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Notes

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1. This is easy to see algebraically as well. When using a probit framework, we estimate a linear model $Y = f(x)$, whose dependent variable can be interpreted as an underlying, unobserved propensity. This variable is transformed into a probability using the cumulative normal distribution function. Adding some independent variable will change Y in a linear way, leading to a nonlinear change in the probability estimate. The effect of a change in Y on the probability estimate will be greatest when the derivative of the cumulative normal function is greatest. The derivative of the cumulative function is just the height of the normal probability density function, and this has a maximum at the midpoint, when the value of the cumulative function is .5.

2. An important weakness of the linear probability model is ordinary least squares predicted probabilities of adoption that are, nonsensically, less than zero. The dichotomous dependent variable in a linear probability results in heteroscedasticity, which normally makes weighted least squares

superior to ordinary least squares for estimation (Aldrich and Nelson 1984). But negative predicted probabilities pose problems for calculating the weights to be used in weighted least squares. Nevertheless, for comparison, we also estimated equation 3 with weighted least squares (setting negative predicted probabilities to .0005 when determining weights); weighted least squares coefficient estimates had the same sign as their ordinary least squares counterparts (but the coefficients for the two terms including RELIGION were substantially weaker).

3. For a linear probability model, ordinary least squares estimates are unbiased; but estimates of the sampling variances are incorrect, so that *t*-ratios cannot be trusted. Consequently, when evaluating the estimates for equation 3, one must focus on the absolute size of the coefficient estimates rather than their statistical significance.

4. See n. 2.

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READING NIETZSCHE AND FOUCAULT: A HERMENEUTICS OF SUSPICION?

To what extent and in what respects does the political thought of Michel Foucault reflect the influence of Friedrich Nietzsche's ideas? In the September 1990 issue of this Review, *Leslie Paul Thiele firmly asserted Nietzschean roots for Foucault's thought. In this* Controversy, *Thiele's claims are disputed by J. Scott Johnson. Johnson argues that Foucault's theory is rooted as much in Marx as in Nietzsche and that Foucault himself refuted Thiele's interpretation. In his response, Thiele takes issue with Johnson's critique.*

Leslie Paul Thiele's "Agony of Politics" (1990a) demonstrates the difficulties inherent in performing a "Nietzschean hermeneutics of suspicion" (Giddens 1982a, 233, see also Gibbons 1987). His thesis presents Nietzsche's glorification of struggle and seeks to impute that glorification to Foucault as well. Rather than digging into, and critically examining, this imputation, Thiele valorizes it. Thiele's essay demonstrates an athletic grasp of Nietzsche's thought but very little critical insight into the relationship between Foucault's work and Nietzsche's. Thiele reads Foucault as if he were reading Nietzsche, and by so doing he erases the differences that mark both Foucault's originality and his debts to other authors. This is a hallmark of suspicious hermeneutics. This is indeed unfortunate, since Foucault's version of the genealogical method seeks to explore differences, not continuity. Still, Thiele's problem is not solely one of method, but performance.

Thiele says, "Foucault's indebtedness to Nietzsche is generally recognized, if seldom analyzed" (p. 909). However, Thiele's essay does little to correct this problem. Thiele does not so much analyze Foucault's debt to Nietzsche as celebrate it. There are two basic problems with Thiele's argument and a third, more general difficulty characteristic of *naive* applications of the hermeneutics of suspicion. First, Foucault explicitly argued against the idea of a simple valorization of struggle such as that presented by Thiele's thesis. Second, Foucault owed at least as much to Marx as he did to Nietzsche, a major point that Thiele neglects to mention. Finally, Foucault argued against the type of commentary that Thiele performs on Nietzsche. Foucault also provided an incisive assessment of what is involved in a hermeneutics of suspicion (Foucault 1979). In other words, the subject of Thiele's essay, Foucault's work, militates against Thiele's method and suggests a different approach. Rather than performing a suspicious commentary, Thiele would have done better to construct his essay as a genealogy of difference.

The first text discussed here, which demonstrates the limitations of Thiele's thesis, is taken from an interview translated in the collection *Power/Knowledge* as "The Eye of Power." Foucault says:

> It just seems to me that the affirmation, pure and simple, of a "struggle" can't act as the beginning

> and end of all explanations in the analysis of power-relations. This theme of struggle only really becomes operative if one establishes concretely—in each particular case—who is engaged in struggle, what the struggle is about, and how, where, by what means and according to what rationality it evolves. In other words if one wants to take seriously the assertion that struggle is the core of relations of power, one must take into account the fact that the good old "logic" of contradiction is no longer sufficient, far from it, for the unravelling of actual processes. (1980, 164)

Neglecting Foucault's explicit statement to the contrary, Thiele argues that Foucault valued struggle for the sake of struggling: "Unending action is required to combat ubiquitous peril. . . . One struggles because the uncontested life is not worth living" (p. 916). Thiele claims that "Foucault's political project is founded on the valorization of struggle" (p. 918). On the other hand, Foucault's genealogies attempt to specify "concretely—in each particular case—who is engaged in struggle, what the struggle is about, and how, where, by what means and according to what rationality it evolves." But we couldn't guess this from Thiele's essay. He remains at too abstract a level; he does not verify his assertions by examining Foucault's practice. Thiele quotes eccentrically from Foucault's eclectic comments on his own work, but does not examine in detail what Foucault actually does. (Note that most of Thiele's references are to interviews.) In other words, we cannot learn from Thiele's essay whether Foucault practices what he preaches. Worse yet, Thiele has paraphrased the wrong sermon. The above quote and Foucault's works suggest Thiele's thesis is misplaced.

The point here is not quantitative; it is qualitative. Foucault's texts are rich, complex, and problematic. The interviews are useful, though secondary, and present interpretive problems of their own. As Foucault and others have noted, an author is but one among many interpreters of a text after it has been written. (And as noted below, there may be different audiences to consider.) These distinctions are ignored by Thiele. The difference between my comment and his article is that I confine my critique to the texts Thiele relies upon most to carry his argument (i.e., the interviews). Rather than developing the issues he fails to address, I show how he fails with the texts he does.

The second basic problem with Thiele's essay is its failure to recognize the important contributions of Marx's thought to Foucault's work. This is understandable—as Giddens wrote, "There seem to be relatively few who are prepared to endure the mental burden of attempting to draw from both Marx and Nietzsche" (1982a, 218). However, in an interview Thiele quotes to make a different point, Foucault made his debt to Marx plain and his reasons for hiding it obvious:

> I often quote concepts, texts and phrases from Marx, but without feeling obliged to add the authenticating label of a footnote with a laudatory phrase to accompany the quotation. As long as one does that, one is regarded as someone who knows and reveres Marx, and will be suitably honored in the so-called Marxist journals. But I quote Marx without saying so, without quotation marks, and because people are incapable of recognizing Marx's texts I am thought to be someone who doesn't quote Marx. . . .[1] It is impossible to write history without using a whole range of concepts directly or indirectly linked to Marx's thought and situating oneself within a horizon of thought which has been defined and described by Marx. One might even wonder what difference there could ultimately be between being a historian and being a Marxist. (1980, 52–53)

Whereas Foucault's debts to Nietzsche are often well marked (but seldom verified and authenticated by his interpreters), his references to Marx, Foucault suggests, are often overlooked.[2] His next response in the same interview makes it clear that his reasons for quoting without attribution were to avoid involvement in a debate not among interpreters of Marx but within the discourse of what might be called "communistologists." Foucault elided references to Marx because Marxism had changed. He complains, "And it's within this general horizon of thought defined and coded by Marx that the general discussion must take its starting point with those who call themselves Marxists because they play a game whose rules aren't Marxist but communistological, in other words defined by communist parties who decide how you must use Marx so as to be declared by them to be a Marxist" (1980, 53). Foucault preferred to argue directly (though often implicitly, and usually without attribution) with the work of Marx rather than participate in this derivative discourse.[3] I suspect that Foucault's appropriation of Nietzsche's thought represents a strategic attempt to avoid this derivative discourse and thus allow Foucault to address directly the problems he believed Marx left unsolved.[4] Had Thiele addressed this possibility, his essay might have been more interesting. Certainly, it would have been more useful.

Thiele can't see Foucault because of Nietzsche. Everything he sees is as Nietzsche saw it first. The prescriptive glasses Thiele wears were ground by Nietzsche. This is the problem that plagues a naive hermeneutics of suspicion: all texts are compared to the works of what Foucault called a "founder of discursivity" (1979, 154), who then takes over the author-function from the author of record. Thiele's reliance on Nietzsche as such a founder (instead of Foucault) shows itself throughout his essay, especially in his brief opening commentary.

I can make this claim stronger by examining the interview that Thiele claims supports his approach. Thiele wants to claim that Foucault can be better understood by understanding Nietzsche and attempts to support this claim with pieces of the following paragraph:

> Nietzsche's contemporary presence is increasingly important. But I am tired of people studying him only to produce the same kind of commentaries that are written on Hegel or Mallarmé. For myself, I prefer to utilize writers I like. The only valid tribute to thought such as Nietzsche's is precisely to use it, to deform it, to make it groan and protest. And if commentators then say I am being faithful or unfaithful to Nietzsche, that is of absolutely no interest. (1980, 53–54)

Thiele begins, however, with a commentary on Nietzsche. He next shows that Foucault quotes many passages from Nietzsche and imports some concepts into his work. There he stops.

Thiele could have performed a useful service by employing Foucault's genealogical method to demonstrate the differences between Foucault and Nietzsche. Then he could have "utilized an author he liked" (meaning Foucault) instead of reproducing Nietzsche to little purpose. This would have allowed him to explain Foucault's attempt to distance himself from Nietzsche: "Nowadays I prefer to remain silent about Nietzsche. When I was teaching philosophy I often used to lecture on Nietzsche, but I wouldn't do that any more today" (1980, 53). Instead, Thiele simply valorized their similarities and continuities, thus concealing their differences. He's already told us the similarities are "generally recognized." Why restate the obvious? Where is his analysis? (see n. 2). Again, Foucault's genealogical method, which Thiele says Foucault "documented as Nietzsche's own," should have suggested a different approach. As Foucault wrote in "Nietzsche, Genealogy, History":

> Genealogy does not pretend to go back in time to restore an unbroken continuity that operates beyond the dispersion of forgotten things; its duty is not to demonstrate that the past actively exists in the present, that it continues secretly to animate the present, having imposed a predetermined form to all its vicissitudes. . . . On the contrary, to follow the complex course of descent is to maintain passing events in their proper dispersion; it is to identify the accidents, the minute deviations—or conversely, the complete reversals—the errors, the false appraisals and the faulty calculations. (1977, 146)

Rather than demonstrating difference, Thiele reasserts an uncontested continuity. This is anti-Foucaultian.

I have shown there are three basic problems with Thiele's argument. First, Foucault explicitly rejected the idea of a simple valorization of struggle. Second, Foucault's use of Nietzsche may have been a strategy to avoid the communistological discourse that had transformed contemporary French ideas on Marx and in any case, Foucault believed he owed at least as much to Marx as to Nietzsche, a major point that Thiele neglects to mention. Finally, Foucault's genealogy—which may or may not be the same as Nietzsche's (it remains to be investigated)—would have been a more appropriate alternative to Thiele's style of commentary, given that Thiele's focus is *ostensibly* Foucault. In other words, Thiele's method does not harmonize with his content. Thus Thiele's naive hermeneutic must be suspicious in another sense. It is neither Foucault's (nor Nietzsche's), nor accurate.

Thiele's analysis could have made a serious contribution to the critique and assessment of Foucault's work had he drawn more on his extensive background study of Nietzsche, focused more on Foucault's genealogies than the interviews, and analyzed the differences between these two authors.[5] We still need to know exactly how Foucault used and "deformed" Nietzsche. How did he make his mentor "groan and protest?" Unfortunately, Thiele does not help us answer this central question.

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Foucault proclaims himself a genealogist. A genealogist does not presume continuity in history but searches for discontinuities. Therefore, my critic tells me, to expose the Nietzschean roots of Foucault's thought, that is to say, to explore the continuity of his thought with Nietzsche's, is antigenealogical, anti-Foucaultian, and ipso facto, wrongheaded. One must expound their differences.

Though I disagree with Scott Johnson's premises, his conclusion is not without merit. The differences between Foucault and Nietzsche should not be ignored. Nor are they in my original essay (Thiele 1990a). Indeed, it was written to explore the political ramifications of their differences. Foucault's Nietzschean roots, I maintain, bring him to an impasse out of which he blazes "his own trail" (p. 909). In describing Foucault's unique journey, I explicitly note that my purpose is not to exhibit Foucault as Nietzsche's faithful follower (pp. 916, 923). I underscore how Foucault's path would be rejected by Nietzsche, that Foucault effectively acknowledges his debt to Nietzsche by plucking at his laurels, and that he creates "anti-Nietzschean theses" (p. 916, 920). Thus, I emphasize Foucault's independence from Nietzsche (p. 919), an independence all the more striking because of the important assumptions and methods shared by the two thinkers—none of which Johnson acknowledges in his critique.

The more important disagreement, however (because of its generality) concerns Johnson's rejection of the investigation of correspondence and continuity in history or intellectual biography as "anti-Foucaultian." In his attempt to keep us faithful to Foucault (a popular but misguided project, I believe) Johnson would restrict us to the dubious task of "demonstrating difference." Yet differences between people interest us because similarities already have been acknowledged. Only in the midst of common values and beliefs are disagreements between thinkers noticeably pungent. Their distinctions are significant only with their sharing the same audience or their involvement with a similar terminology, methodology, set of assumptions, or project. The marks of difference have salience only against a relatively uniform background. Genealogy can no more afford to ignore these facts than any other interpretive endeavor. Demonstrating difference, in itself, serves little purpose.

This point seems obvious—indeed banal—regarding most other figures. Describing in detail the chasm of discontinuity between the thought and action of Helmut Kohl and Joan of Arc, I presume, would be a singularly unfruitful enterprise. Their numerous differences would remain trivial until one somehow demonstrated an *interesting* correspondence in the words or deeds of the German chancellor and the Maid of Orléans. In this case, the fact that both figures established themselves in the pursuit of unity and sovereignty for their respective nations appears an unredeeming parallel. There is a glut of disparities and congruities in the world. One must be selective. In other words, the importance of differences discovered is always in direct proportion to the importance of continuities established. This point is no less true for Nietzsche and Foucault.

I have elsewhere argued (1990b) that Nietzsche is often misrepresented as one who celebrates the dispersion and destruction of the subject without exhibiting equal concern for its ordering and unity. So do I find Foucault often misrepresented as one who suggests that genealogical accounts of social phenomena evidence only accident and discontinuity—that, as Johnson straightforwardly puts it, "Foucault's version of the genealogical method seeks to explore differences, not continuity." Johnson acknowledges Foucault's description of genealogies as following the "complex course of descent" of events in their "proper dispersion." Importantly, then, there is indeed a course laid out by the genealogist. Moreover, its discontinuities become remarkable only because a more or less continuous historical trajectory has been distinguished in the first place. Indeed, a courseless account of history would elude a subject matter and probably escape intelligibility; for without the glue of continuity to hold the story of disruption and dispersion together, one would be hard pressed to discover a topic, arrange its formulation, and structure its argument. Foucault's genius is evidenced not in the pasting together of unrelated anecdotes but in illustrating that historical coherencies are formed from the confluence of multiple strategies and tactics of power and knowledge. History, he shows, is not the product of grand narratives with teleological movements but of diverse struggles that nonetheless become organized into coherent (that is to say, more or less continuous) patterns of domination, subjectification, and government. To assume anything less is to do much disservice to Foucault.

Foucault himself assumes some responsibility for the "dogma" of discontinuity that arose soon after his writing *The Order of Things* (1970), owing in part to his exaggeration of the discontinuities of history for "pedagogical purposes" (1988b, 99–100). But he regrets that "many readers saw no further. They failed to see that the whole work of the book consisted precisely in setting out from this apparent discontinuity . . . and trying, in a way, to dissolve it" and concludes, "My approach, therefore, was quite the opposite of a 'philosophy of discontinuity' " (pp. 99–100). We may choose not to forgive Foucault for his negligence in fostering the dogma of discontinuity; but in the hope that this dogma will wither away, we would be well advised to forget it. In so doing, the words of Martin Heidegger might be remembered, words pregnant with unexpected meanings that cannot here be explored: "We can only say 'the same' if we think difference. It is in the carrying out and settling of differences that the gathering nature of sameness comes to light" (1971, 218–19).

"The Agony of Politics" offers an account of Foucault's intellectual debt to Nietzsche. Johnson thinks this anti-Foucaultian. It is, in his view, an example of a Nietzschean "hermeneutics of suspicion"; and he prescribes an alternative account highlighting Marx's influence. (Would not this proposal amount to making the same mistake twice, now substituting Marxian continuities for the spurned Nietzschean ones?) I think the enterprise worthwhile, particularly because an examination of Foucault's relation to Marx best highlights Foucault's indebtedness to Nietzsche. Of course, what Foucault owes to Marx depends a great deal on what ideas Marx himself (as opposed, perhaps, to Marxism) is credited with. If, like Engels, we hold Marx's greatest "discoveries" to be historical materialism and the theory of surplus value, then Foucault owes Marx the honor of being a worthy antagonist but one who quickly ceases to orient Foucault's work. Foucault, early on in his career, rejects both the tenets of historical materialism and the focus on production as the key to social development, in their place adopting a Nietzschean enterprise and methodology. Genealogy replaces the grand historical narrative; the focus on power/knowledge (and, in his later work, on modes of subjectification) replaces the Marxian focus on social production. Foucault, who held the chair in the History of Systems of Thought at the Collège de France, acknowledges the debt owed by all later historians to Marx's genius. Foucault's debt, however, is largely epaid in a fierce competition with Marx for theoretical space. It follows that Foucault and Marx unavoidably come into contact. But almost all Foucault's skirmishes with Marx find him courting Nietzsche as a comrade in arms.[6] Johnson acknowledges this point, but the acknowledgment seems inconsistent with the thrust of his critique. Let me explain by reference to the type of hermeneutics I am accused of practicing.

The phrase *hermeneutics of suspicion* originates with Paul Ricoeur (not, as one might conclude from Johnson's critique, with Anthony Giddens).[7] It does not mean (as Johnson believes it does) the erasure of an author's originality through a demonstration of that author's complete indebtness to a predecessor. Even if it did, it could hardly be applied in the present case, for Foucault writes that Nietzsche establishes the disappearance of the author as an irreducible foundation and primordial origin of the work (1977, 121). By *hermeneutics of suspicion* Ricoeur means to characterize the interpretive exercise that searches for a hidden meaning in the symbols, actions, and words of a subject in an attempt to unmask "false consciousness." The "masters of suspicion" for Ricoeur include Marx, Nietzsche, and Freud. Freud is singled out as the "paradigm" of this reductive hermeneutics (Ricoeur 1974, 18; Ricoeur 1981, 34); but all three thinkers are proposed as "protagonists of suspicion who rip away masks and pose the novel problem of the lie of consciousness and consciousness as a lie" (1974, 99). The hermeneutics of suspicion privileges the interpreter with the key to truths unavailable to the consciousness of the subject. Marx's critique of ideology as that which inverts reality, obscuring the nature of class society and the workings of historical materialism; Nietzsche's testament that the will to power, unbeknown to its subjects, underlies all values, structuring life itself; and Freud's investigations into the "guile" of consciousness, revealing, through the interpretation of dreams and other channels to the unconscious, the inescapably repressive effects of cultural norms—all these interpretive enterprises are held by Ricoeur to constitute reductive demysifications.

I disagree with Ricoeur's characterization of Nietzsche. Nietzsche's writings on the will to power are not, I think, examples of a hermeneutics of suspicion. One cannot deny that Nietzsche leaves himself vulnerable to such a charge; yet it seems a gross misreading of Nietzsche simply to dismiss his perspectivism as dogmatic (as Ricoeur does) and thus situate him with Marx and Freud as another nonskeptic, "free[ing] our horizon for a more authentic speaking, a new reign of truth" (Ricoeur 1974, 149).

Foucault also rejects Ricoeur's judgment of Nietzsche (particularly as it pertains to the later Nietzsche; see Foucault 1977, 141–42). Indeed, in his essay "Nietzsche, Freud, Marx," Foucault fundamentally contests the Ricoeurian thesis. He argues that in all three thinkers, especially Nietzsche and "to a lesser degree" Marx, a final reign of truth is not inaugurated; rather "interpretation has at last become an infinite task" (Foucault 1986, 3). For Nietzsche, Foucault emphasizes,

> interpretation is always incomplete. . . . There is nothing absolutely primary to interpret, for after all everything is already interpretation. . . . There is never, if you like, an *interpretandum* that is not already *interpretans*, so that it is as much a relationship of violence as of elucidation that is established in interpretation. . . . It can only seize, and violently, an already present interpretation, which it must overthrow, upset, shatter with the blows of a hammer. . . . A hermeneutic [of suspicion] that in effect falls back on a semiology believes in the absolute existence of signs: it abandons the violence, the incompleteness, the infinity of interpretations in order to enthrone the terror of the index and to suspect language. Here we recognize Marxism after Marx. On the contrary, a hermeneutic that wraps itself in itself enters the domain of languages which do not cease to implicate themselves, that intermediate region of madness and pure language. It is there that we recognize Nietzsche." (1986, 3–5)

Foucault is less gentle to Marx in other writings where Nietzsche himself becomes Foucault's hammer. Foucault posits Nietzsche as shattering the quest for a reductive demystification that animates much of the nineteenth century, including Marx's thought. Foucault maintains that it is Nietzsche who undermines this project of discovering, once and for all, man's anthropological truth (1970, 263). Marx, on the other hand, does not escape that regime of "philosophy from Hegel to Sartre [that] has essentially been a totalizing enterprise, if not of the world or of knowledge, at least of human experience" (Foucault 1989, 38). Hegel and Marx acknowledge the death of God, but (as Nietzsche would say) they are still haunted by God's shadows. History, as the teleological tale of man's final (statist or antistatist) redemption, is the specter haunting most nineteenth-century thinkers. Marx, Foucault insists, is no exception:

> At the deepest level of Western knowledge, Marxism introduced no real discontinuity; it found its place without difficulty . . . within an epistemological arrangement that welcomed it gladly . . . and that it, in return, had no intention of disturbing and, above all, no power to modify, even one jot, since it rested entirely upon it. Marxism exists in nineteenth century thought like a fish in water: that is, it is unable to breathe anywhere else. (1970, 261–62)

Thus, in contrast to Nietzsche's efforts in restructuring modern thought, Foucault characterizes the controversies spawned by Marxism as having "stirred up a few waves and caused a few surface ripples; but they are no more than storms in a children's paddling pool" (p. 262).

Foucault holds that our age may be characterized as attempting to flee Hegel through either Marx or Nietzsche (1972, 235). His rejection of the Marxist route, despite his early flirtations with it, is based on the conviction that despite turning the master on his head, Marx never really escapes Hegel's gaze.[8] Hegel inverted is still Hegel. On the other hand, Nietzsche subverts Hegelianism, undermining its lure by questioning its motives. Foucault performs the same service for Marxism.

In short, the importance of Nietzsche for Foucault, as I note in my essay (p. 915), is precisely that Nietzsche insists (pace Ricoeur) that there is no reign of truth, anthropological, historical, or otherwise that is not already the product of power and struggle. A hermeneutics of suspicion is misconstrued, both Nietzsche and Foucault maintain, because there is no God's-eye view of history or of the human soul from which truth may be calibrated. For these reasons Foucault refuses to speak in terms of ideology. To study ideology is to investigate false consciousness; and such an effort implies a belief in the existence and desirability of a true consciousness (e.g., a proletarian consciousness) that could escape the distorting effects of power. But the webs of power constitutive of social life are, Foucault insists with Nietzsche, omnipresent and inseparable from our identities—hence Foucault's affirmation of the interminable struggle of competing interpretations and their accompanying politics. The alternative to such agonism is simply too dangerous. For it is only from the vantage point of true consciousness and its canonical language that the "terror of the index" can be maintained. Thus, in direct opposition to the Marxist position, Foucault holds that in the modern age the regime of truth and power should not be considered "merely ideological or superstructural" (1980, 133). It is fundamental and fundamentally contestable.

All this is not to say, however, that Nietzsche is the sole force structuring Foucault's thought. If we wish to dig deeper into Foucault's intellectual background, however, it is not to Karl Marx but to Martin Heidegger that we must turn. In October 1982, answering a question about the intellectual influences upon his thought, Foucault offers only two names: Heidegger and Nietzsche.[9] In his final interview, conducted in the spring of 1984, he further reveals this debt:

> For me Heidegger has always been the essential philosopher. I began by reading Hegel, then Marx, and I set out to read Heidegger in 1951 or 1952; then in 1952 or 1953—I don't remember any more—I read Nietzsche. I still have here the notes that I took when I was reading Heidegger. I've got tons of them! And they are much more important than the ones I took on Hegel or Marx. My entire philosophical development was determined by my reading of Heidegger. I nevertheless recognize that Nietzsche outweighed him. . . . My knowledge of Nietzsche certainly is better than my knowledge of Heidegger. Nevertheless, these are the two fundamental experiences I have had. It is possible that if I had not read Heidegger, I would not have read Nietzsche. I had tried to read Nietzsche in the fifties but Nietzsche alone did not appeal to me—whereas Nietzsche and Heidegger: that was a philosophical shock! (1985, 8–9)

I shall not investigate here Foucault's relationship to Heidegger in general or even the importance of Heidegger with regard to Foucault's reading of Nietzsche. At this time and for the purpose of this reply, Foucault's own words must suffice to buttress the assertion that Heidegger, not Marx, should be considered Foucault's greatest intellectual creditor after Nietzsche.

Perhaps the most difficult of Johnson's charges to parry is that my argument about Foucault's valorization of struggle "remains at too abstract a level," never substantively engaging Foucault's own genealogies to investigate the nature of particular struggles and thus never discovering whether "Foucault practices what he preaches." In fact, I examine Foucault's reflections on the relation between his intellectual activities and practical politics; I note that his thinking and writing were integrated with his political struggles on certain issues; and I attempt to show that Foucault's valorization of struggle does not lead to an abandonment of all means of evaluating particular struggles, arguing on the contrary, that Foucault's position allows us a criterion for judgment. Be this as it may, I would not want to deny that my work evidences conceptual analysis and its inherent abstraction. "The Agony of Politics" is not meant to serve as a substitute for, or an exemplar of, Foucault's detailed historical analyses. It is, to be sure, less a genealogy than an interpretation of the theoretical undercurrents structuring the thought of a genealogist.

Nonetheless, were the essay much broader in scope and more rigorous in method, it might be construed as a genealogy of Foucault's thought. It attempts to orient itself to Foucault's intellectual struggles in the same manner that Foucault orients himself to particular historical struggles. The purpose of genealogy is to discover the ambiguous forces and the corresponding forms of resistance that actuate individuals, institutions, and events—not along some predetermined path but in a direction that in retrospect is identifiable. Foucault advocates that one who "wants to take seriously the assertion that struggle is the core of relations of power" must establish "concretely—in each particular case—who is engaged in struggle, what the struggle is about, and how, where, by what means and according to what rationality it evolves" (1980, 164). In my essay I attempt this exercise with respect to Foucault's thirty-year struggle with Nietzsche.

Referring to the same quotation, Johnson believes he has discovered an "explicit" demonstration that Foucault's political project is not founded on the valorization of struggle. In fact, what is established (Johnson quotes it) is that "the affirmation, pure and simple, of a 'struggle' can't act as the beginning and end of all explanations in the analysis of power-relations." This I happily iterate. Foucault's *own* affirmation of struggle explains little, if anything, about the formation of *particular historical* relations of power. It does, nonetheless, explain a great deal about Foucault and his work.[10]

In sum, Johnson's primary objection to my commentary is that I have allowed Nietzsche to step between Foucault and myself. This blockage blinds me to certain things (Marx's influence) and disables me from others (genealogical investigation). Thus, I end up *re*-presenting Nietzsche instead of adequately describing Foucault. I have tried to show why I find this criticism unfounded and misconstrued. But there *is* an admission to be made: commentary on Foucault is inherently problematic. One cannot deny that Foucault wore his masks (Nietzschean and other) to escape identification as much as to allow it. His own words, again, best tell the tale: "I am no doubt not the only one who writes in order to have no face. Do not ask who I am and do not ask me to remain the same: leave it to our bureaucrats and our police to see that our papers are in order. At least spare us their morality when we write" (1972, 17). I took on the task of putting Foucault's papers in a particular order.[11] Commentary, at least that exhibited by my essay, necessarily partakes of a bureaucratic procedure. On its own, this interpretive work is not an especially Nietzschean or Foucaultian endeavor; yet an implicit Nietzschean and Foucaultian assumption structures the argument, and it is now best made explicit.

Foucault might well have groaned and protested at the deformation to his thought suffered in my attempt to demonstrate precisely how Nietzsche groans and protests in Foucault's own grip. Alternatively, were he to adopt a genealogical attitude with its focus on the uses discourse is put to and its assumption of the inherently problematic nature of authorship, we might hear from him "little more than the murmur of indifference: 'What matter who's speaking?' " (Foucault 1977, 138). Nonetheless, distortions to a thinker's work, unavoidable in any worthwhile commentary, are best contended. I gladly accept challenges to my interpretation and believe on purely scholarly grounds to be able to defend myself against them. But beyond that, the general charge that I have done violence to Foucault, molding him to my own image, is no burden. We should all be spared such morality when we write. With the worldly Lady Macbeth (though for other reasons) we may conclude, "Things without all remedy should be without regard."

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Notes

1. I omit "When a physicist writes a work of physics, does he feel it necessary to quote Newton and Einstein? He uses them but he doesn't need the quotation marks, the footnote and the eulogistic comment to prove how faithful he is being to the master's thought. And because other physicists know what Einstein did, what he discovered and proved, they can recognize him in what the physicist writes" (Foucault 1980, 52–53) because Foucault makes a distinction between scientific discourse and the type of discourse founded by Marx (see Foucault 1979).

2. The standard interpretation of Foucault is Dreyfus and Rabinow's *Michel Foucault* (1983). They put to one side all tests of validity in Nietzschean interpretation when they write: "This is not to say that Foucault is in full accord with Nietzsche—whatever that could mean for a thinker as complex, elusive and profoundly antisystematic as Nietzsche. We plead neutrality concerning the textual accuracy of Foucault's reading. Nietzsche interpretation, a flourishing industry in France in recent years, is a field of danger and strife which we will leave to others, more fully armed; our concern is with Foucault" (p. 106). Thiele may be more fully armed, but he is only shooting blanks; for his concern is not with Foucault at all.

3. Colin Gordon, in the afterword to *Power/Knowledge*, discusses Foucault's break with politically correct "Marxism" as follows: "One might say that the trouble with Foucault's work was that its originality was in inverse proportion to its utility for Marxism. Now factors such as the rather obvious discrepancy between the events of 1968 and after, and the revolutionary time-table of the Communist party, both made it increasingly difficult for the organized left to impose on its loyal intellectuals the strict conditions of service customary in the era of Zhdanov, and opened up the possibility on the Left for a reconsideration of some of the problematic features of Marxism" (1980, 232). The point I want to make (which I think is consistent with both Foucault's approach and Gordon's discussion) is that Foucault may have been less useful for Marxism but more useful for interpreters of Marx. A number of discourses share the label of Marxism uneasily.

4. Indeed, Foucault's reasons for moving to Nietzsche might be the same as those of some of the recent French interpretors of Nietzsche described in Dreyfus and Rabinow 1983 (see n. 2).

5. Thiele's new work, *Friedrich Nietzsche and the Politics of the Soul* (1990b) may have enough of an exegetical foundation for the task I am suggesting.

6. Foucault's concern with critique rather than programmatic politics situates him in the proximity of critical theory and its Marxist heritage. Yet we must acknowledge, with Barry Smart, that "although the work of Foucault may be situated in relation to the limits and limitations of Marxism, its relationship to the Marxist tradition is an external and generally critical one" (Smart 1983, 136). Thomas McCarthy's recent appraisal of Foucault's thought in comparison to that of the Frankfurt School reaches the same conclusion. Foucault's work and that of the Frankfurt School, McCarthy holds, "occupy neighboring territories in our theoretical world, [but] their relations are rather combative than peaceable. Foucault's Nietzschean heritage and the Hegelian/Marxist heritage of the Frankfurt School lead them to lay competing claims to the very same areas" (1990, 441).

7. Giddens is a curious source for Johnson to cite; for Giddens is acutely aware of Foucault's "displacement of Marx by Nietzsche" (1982b, 221). Giddens also acknowledges that there is a *Marxian* "hermeneutics of suspicion" to be confronted (p. 223). Johnson appears unaware of this problem.

8. Foucault explicitly denies ever having been a Marxist (1988b, 22), notwithstanding his joining the French Communist party for several months in the postwar years and his tutelage by Louis Althusser.

9. First, Foucault mentions Heidegger as being prominent. He then adds: "Nietzsche was a revelation to me. I felt that there was someone quite different from what I had been taught. I read him with a great passion and broke with my life, left my job in the asylum, left France: I had the feeling I had been trapped. Through Nietzsche I became a stranger to all that" (1988a, 13).

10. It is worthwhile noting that with the exception of certain of Marx's historical writings, Foucault criticizes Marx and the Marxists precisely for ignoring the characteristics of particular struggles: "They speak of the 'class struggle' as the mainspring of history, they focus mainly on defining class, its boundaries, its membership, but never concretely on the nature of the struggle" (1988b, 123).

11. I do not know what to make of the charge that this effort fails as a "serious" contribution to scholarship because, in part, it neglects Foucault's genealogies and focuses on his interviews. In my essay references to Foucault's interviews are roughly equal in number and importance to references to his essays and books. Johnson, on the other hand, draws most of his support from Foucault's interviews: his references to interviews outnumber those to Foucault's written works three to one. But count-

ing references misses a more important point. Foucault gave interviews, many of them of utmost interest, to aid us in understanding his work. There seems to be no reason to reject their offerings. Indeed, in his essay "What is an Author?" he challenges the standards of scholarship that exclude or devalue such information out of hand (1977, 118-19).

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Forthcoming in September

The following articles, research notes, and controversies have been tentatively scheduled for publication in the September 1991 issue:

Harold D. Clarke, and Nitish Dutt. "Measuring Value Change in Western Industrialized Societies: The Impact of Unemployment." A research note.

David C. Paris. "Moral Education and the 'Tie that Binds' in Liberal Political Theory."

Keith T. Poole, Howard Rosenthal, and Kenneth Koford. "On Dimensionalizing Roll-Call Voters in the U.S. Congress." A controversy.

Dennis Quinn, and Robert Shapiro. "Business Political Power: The Case of Taxation."

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Harrison R. Wagner. "Nuclear Deterrence, Counterforce Strategies, and the Incentive to Strike First."

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THE POLITICS OF EQUALITY ACROSS THE GLOBE

Losing Face: Status Politics in Japan. By Susan J. Pharr. Berkeley: University of California Press, 1990. 226p. $25.00.

Like almost every book on Japanese politics, this one is about nature versus nurture or (if you will) culture versus rationality. Pharr has written a book that comes down on the side of nurture and culture. Ostensibly, the book is about "how struggles over equality are waged in Japan and how authority responds to them" (p. x). Additionally, the book, she claims, is also about social conflict. Demands for equality, she argues, present special problems in a society where demands involving the "assertion of self, claims of entitlement, demands for oneself and one's group fly in the face of the Japanese ideal model of protest" (p. xii).

These two objectives are pursued by examining three cases of protest over status. One is a case seen as stemming from age–seniority differentiation; the second describes the protest of a group of high school students belonging to a traditional pariah class, the so-called *eta* or *burakumin;* the third case of protest concerns a small group of women civil servants in Kyoto over the continued expectation that they should brew, pour, and serve tea to their male coworkers.

Leaning heavily on Charles Tilly's resource mobilization approach, Pharr shows rather nicely how each protest utilized strategies within the available social repertoire. Leaving this level of analysis, she then proceeds to argue that the systematically limited levels of conflict and successes achieved by these three protests are characteristic of protests in Japan generally. This leads Pharr to conclude that there exists a widely accepted "ideal model of protest" (pp. 36–37), which includes the norms that protests should occur only under extreme deprivation or injustice and be guided by high moral purpose and that protestors should show total commitment, self-sacrifice, and constraint in their tactics.

Where does this ideal come from? The argument seems to be that it stems from the Japanese habit of privatizing social conflict (pp. 207–8), that is, the pattern of shunting social protests off into the private realm. Privatization, in turn, arises as a consequence of the value and norms of conflict avoidance in Japanese society (p. 208). This is a strategy, she argues, that has emerged and evolved over centuries of feudalism and has been adapted to the needs of a modern industrial democracy. She notes, with some admiration, that this helps to account for the high degree of stability in postwar Japan (pp. 12, 210–11, 218–20). The expression of this admiration at both the beginning and end of the study suggests to us that this is a book as much about social control as about protest and equality in Japan.

In short, the ideal model of protest has its roots in traditional values that have persisted despite enormous changes in institutional arrangements. This has a nice, logical ring to it. There are, however, some difficulties with this argument. If this pattern had evolved during the long feudal period, why was it incapable of constraining the great protests that led to the Meiji Restoration or those that followed the restoration all the way into the late 1920s?

The greater success rate of constraint in the postwar period suggests that success was due to structural change after World War II rather than a function of cultural continuity. Indeed, one might argue that the manner in which economic development occurred in the postwar period was just such a change. It provided the basis for encompassing an ever-larger portion of Japanese society within the realms of organizations dominated by bureaucratic labor markets. These dominated, in turn, a vast array of smaller organizations whose contingent existence allowed them to enforce hierarchical social relations. Political and economic elites were thus able to enforce social harmony not by consensus but by the coercive nature of the organizational relationships. At the same time, elites were once again able (as they had been at other times) to call upon the old clichés of social harmony about which their coercive power was once again able to give a sense of reality. This is not quite the same thing as consensus; and it is neither egalitarian nor democratic (as Pharr would have us believe it is; see p. 208).

This alternative argument may also be used to explain the low level of conflict in Japanese protest: perhaps people avoid high levels because the costs are high, not because they value conflict avoidance. When this organizational coercion was much lower, people engaged in profound social protest. This argument may not be right, but we ought to be told why it is not. This study provides valuable insights but fails to the degree that it cannot tell us why we should accept this version rather than another.

BERNARD S. SILBERMAN

University of Chicago

Preferential Policies: An International Perspective. By Thomas Sowell. New York: William Morrow, 1990. 221p. $17.95.

Race is not solely an American dilemma. Nor are preferential policies a uniquely U.S. enthusiasm. Most scholars and journalists partially understand the first point; but the second is, at best, only dimly perceived. The news of riots and suicides in India recently have given the public a glimpse of affirmative action problems abroad. But as the economist Thomas Sowell makes clear in his most recent book, India is not the only ethnically divided country to have adopted hiring and other quotas for "disadvantaged" groups. Government-mandated preferential policies can be found in Malaysia, Nigeria, Sri Lanka, South Africa, and other nations as well. And while the settings vary, the results are everywhere much the same, Sowell argues. Different folks, same strokes.

The public now equates preferential policies with affirmative action for blacks and other minorities. In fact, as Sowell makes clear, that is only one model. In the Jim Crow South, laws forced often-reluctant white-owned businesses to prefer whites over blacks. (Not all businessmen wanted to pay the higher costs that were the consequence of a distorted market.) In other settings (Malaysia and Nigeria, for instance) preferential politics have been the means by which the political rich have stripped wealth from an economically more successful minority. A numerical majority has used its political power to rob a skilled minority of its economic advantage.

Preferential policies in their more familiar form of preferences for minorities in a majority-dominated economy originated in India. In the 1930s the British colonial government agreed to a system of reserved legislative seats and other communal benefits for untouchables. The system subsequently grew by leaps and bounds. But whether in India, Sri Lanka, Israel, or the United States, Sowell argues, the results have been nothing short of disastrous. In education, preferential policies have led to high attrition rates and substandard performances, often in the less demanding and less promising fields. In employment, preferences have helped those who needed no help and harmed those truly at the bottom. And on the streets and in politics, racial and ethnic tension has been further exacerbated by policies that pit one group against another. Only in South Africa, Sowell suggests, has a group risen from poverty to prosperity as a result of group-based benefits; the Afrikaners, once poor, have been rescued by a preferential policy that ruthlessly sacrificed nonwhite interests.

It is an ugly picture, generally obscured by total indifference to the costs and benefits of affirmative action, Sowell contends. Moral grandstanding substitutes for empirical inquiry. Proponents create a web of illusions. They pretend, for instance, that preferential policies will only be temporary, that they will remain confined to one "disadvantaged" group, and that their effect will be the intended one. They also pretend, most importantly, that they can identify who deserves what. In fact, Sowell argues, we can point to statistical disparities (blacks earning less than whites on the average, for instance), but we cannot satisfactorily explain them.

We don't know enough to do so. That knowledge is impossible to come by. Too many variables (some very subtle) determine the interest, skill, and performance of individuals from different groups. Preferential policy proponents assume that in an ethnically and racially "fair" world, groups would be proportionally represented in the science labs, on the baseball field, and in legislative halls. There is no evidence for such a proposition. In fact, all the evidence points in quite the opposite direction. Ethnic inequality is literally universal;

both in the presence of discrimination and in its absence, different groups fall into different occupational and other patterns. That's the norm, somehow seen as anomalous.

Contemporary society has a moral duty to eradicate the lingering effects of past oppression, affirmative action proponents claim. Sowell says, in effect, save your moral energy for another cause: you've got this one wrong. Guilt isn't collective or inheritable; blacks who today benefit from yesterday's wrongs are the arbitrary recipients of a windfall to which only ancestry has entitled them. And to argue that compensatory policies offset the contemporary effects of past wrongs is to confuse causation with morality. Perhaps today's high black teenage pregnancy rate is the consequence of historic discrimination. We don't know and can't know. Such matters are beyond the grasp of the most skilled historian or social scientist.

What we can ask (Sowell suggests) is, What sorts of incentives will likely lead to what results? How do we get where we want to go, perhaps not tomorrow but at least down the road? And if we cannot agree on what our policy should be, at least we can decide to talk sense. For accusatory rhetoric, definitional games, and specious facts, we can substitute a commitment to empirical evidence. If we do that, the result just might be real progress for groups that now seem almost hopelessly behind.

The trouble is that, by Sowell's own admission, hard empirical evidence has not so far made much of an impression on those who disagree with him. He gives some of the reasons himself. Preferential policies, for instance, are a seeming quick fix; and they allow intellectuals and politicians to feel good at relatively little cost. But more important, perhaps, is a reason Sowell doesn't mention. The two sides are separated by a fundamental disagreement over a matter that cannot be settled with more facts: they don't view U.S. society through the same lens. Affirmative action opponents are optimists; they trust their fellow citizens to be generally fair. Proponents see continuing and pervasive racism. They see the deck as stacked against members of minority groups. And they want admissions, employment, promotion, and other processes carefully policed. It's but a short step from such distrustful scrutiny to an insistence on statistical parity, on getting the numbers right. Quotas are the policy of hard-core skeptics, of whom there are many.

Undoubtedly, Sowell would sharply distinguish himself from a writer like Shelby Steele, who looks inward to grasp the outside world, starting with his feelings to get at the facts about affirmative action and other such subjects. Autobiographical musings, Sowell might argue, do not address social science questions. Yet Sowell's own writing has an autobiographical cast to it. His book is laced with the pain of a man whose ironclad logic is dismissed by those who talk unctuously, impugn motives, and believe that a passionate commitment to social justice trumps hard data and cold reason. *Preferential Policies* is a solid book, full of startling and important evidence. It is also, in its quiet way, an angry book. The emotion that Sowell would banish from affirmative action discussions runs noiselessly through his own work.

ABIGAIL THERNSTROM

Boston University

A Secretary and a Cook: Challenging Women's Wages in the Courts of the United States and Great Britain. By Steven L. Willborn. Ithaca: Industrial & Labor Relations, 1989. 214p. $32.00 cloth, $14.95 paper.

In *A Secretary and a Cook* Steven Willborn has explored a subject of great significance to the lives of working women and men. Using the vehicle of tracing two women's lawsuits for pay equity in, respectively, the United States (the secretary) and Great Britain (the cook) Willborn suggests that the differences in legal process may affect not only the substance of the law that emerges but also the "rewards" reaped by the litigants. While the women employees of the state of Washington ultimately lost their class action suit before the Ninth Circuit Court of Appeals (after prevailing in a U.S. district court), the state nevertheless implemented a form of pay equity, and many women in traditionally "female" jobs received pay raises. In ironic contrast, the cook's case for pay equity in England, where they do not utilize the "class action" suit, was victorious in principle but resulted in no direct material benefit for women workers.

Contrasting the two cases by interleafing the litigants' stories in the first eight chapters of the book is an interesting approach to analyzing pay equity, but the author has not accomplished it in an entirely satisfying manner. Several observations suggest this conclusion. First, the stories tend to be filled with exceptionally precise detail about factors that are not of great consequence to understanding either the issues involved in the suits or the significance of the sources and processes of the law in the two societies. For example, when discussing the case of Julie Hayward (the cook) Willborn repeatedly tells us about the *situation* in which the case was set and to which jobs hers might be compared. In contrast, he offers little analysis of the great significance for pay equity in Europe of the fact that the command that it apply to Hayward's case came not from England's own law but from the Council of Ministers of the European Economic Community under its 1975 interpretation of the Treaty of Rome. In later chapters of the book, the author displays tables of female-to-male earnings in a variety of countries to demonstrate the ubiquitousness of sex as a predictor of compensation. More glaring in these 1980 data, however, is the relative failure of the United States (60%) to achieve gender equality in compensation in comparison with European countries, especially Sweden (90%) and Italy (83%). Given that Willborn's larger goal in telling these stories (as is evident in the last three chapters of the book) is to assess pay equity as a strategy for redressing gender inequalities in the workplace, he might have seized the opportunity to offer us some analysis of the structural and/or cultural factors that would explain the poor performance of the United States in this respect.

Also limiting the effectiveness of his method for telling the pay equity story is that while Willborn uses the two women's names constantly (I assume as a device for making the reader see the really human drama of the events), the book tells us almost nothing about either of these women: the complexity of their lives, the effect of their lawsuits on their professional and personal experiences, how significant their goals and views were in framing either litigation, and so on. Essentially, one gets the sense that the women have become metaphors by which to humanize women's struggles that were handled, shaped, and ultimately completely controlled by men.

In part 2 Willborn more effectively educates his readers about the nature of pay equity issues, consequences, and limitations. He offers an excellent defense of the concept of pay equity as *inherent* in the antidiscrimination principle of Title VII of the 1964 Civil Rights Act, rebutting the criticism that it is "a novel and risky venture" (p. 141) as well as the contention that employers should not be held accountable for pay inequities that reflect the market. He is, similarly, very persuasive in demonstrating the weakness of the employer's defense that factors "other than sex" to explain pay inequities would include, for example, union agreements that result in a devaluing of "women's work." Not only are unions also bound by Title VII, but to allow for such defenses is to allow for the continuing use of gender as a *discriminating criterion* in labor compensation.

Finally, Willborn has also offered important critiques of a variety of the economists' conclusions about pay equity. He notes, for example, that many of these critics fail to see the dynamism of a labor situation. The observation of some economists that 18% of the wage gap between men and women can be explained by the greater "human capital" of men (which they assume translates into greater productivity!), is countered by Willborn's observation that perhaps women would be encouraged to hone their skills further, that is, heighten their "human capital" if they felt more fairly compensated. The same analysis would also hold true, Willborn suggests, for the potential effects of pay equity on the differences between men and women in terms of numbers of hours worked, likelihood of leaving a job, and so on. And to economist Paul Weiler's conclusion that because the net effect of pay equity would be only an overall 2%–3% closing of the male-female wage gap, it is a fruitless pursuit, Willborn responds that the *average* gain figures mask the fact that *some* women's professions would see pay raises as high as 40%. And this, too, could have its own dynamic impacts on labor economics.

While Willborn has met the economists well on their own turf, there is, nevertheless, in his defense of the principle of pay equity a sense in which he has not successfully captured the broader essence of the pay equity movement. Willborn asserts that pay equity (or, as he and perhaps most writers conceptualize it, "comparable worth") means "equal pay for jobs that are of comparable value to the employer." He further defines the concept of "value" in terms of the "skill, effort and responsibility necessary to do the job" (p. 127). One is immediately led to ask why "value" *to an employer* would necessarily be defined in this matter. Perhaps jobs requiring little skill actually generate relatively more "value" for an employer in the form of income than do those requiring high levels of skill, effort, and responsibility. Would his theory of pay equity allow an employer to make just this claim in rebutting a charge of sex discrimination?

In Willborn's defense, his definition of comparable worth is the standard application of the concept by the sympathetic members of the legal profession. The problem is that it fails to acknowledge its own internal tension between an employer's basis for determining value and the definition of value that is a function of skill, responsibility, and effort. While those *unsympathetic* to the legal and ethical claims of pay equity would be most likely, I would imagine, to make this observation, the observation leads to what I see as a core element of the feminist movement for pay equity. The debate is not just about whether the gender composition of specific jobs can be *isolated statistically* as *the* reason why, for example, registered nurses earn less than truck drivers. It is not just about peeling away all of the other economic hypotheses that *could* explain pay differences between men's and women's work and concluding that just the (alleged) 2%-3% difference that is left may be gender-based. It is about a social reexamination of *value*. It is also simultaneously about analyzing the gender relevance of the criteria that make up traditional economists' notions of "skill," or "responsibility," or "human capital." It is interesting to note, for example, that *supervising others* is a regular plus on job value scales as reflecting added "responsibility." Not only does this reflect the valuing of hierarchy, which in the work of Carol Gilligan and others is more highly associated with men than women; but for women professionals, who are often provided fewer support staff than are men and consequently must do more work themselves, such a criterion is not immediately or intuitively persuasive as a measure of value. Also, job value surveys typically classify supervising children not as "supervision" but as "service." As is well documented, caring for children is an almost universally female occupation.

In brief, it is insufficient to look for "room" for pay equity in the gap left after the economists have massaged the labor value data with allegedly gender-neutral hands. Pay equity, if meaningful, is far more fundamental. It is a part of an analysis of work and assesses it *as a system* that devalues women. Pay equity asks not only to have the skills of women that are as useful in society as the skills of men compensated equivalently but also that in calculating value, the experiences and *values* of women are equally integrated into the calculation process. While Willborn's study of the secretary and the cook may encourage the reader to consider this broader context of pay equity, the work does not itself fully address these more fundamental problems of defining the value of women's work.

GAYLE BINION

University of California, Santa Barbara

The Reconquest of Montreal: Language Policy and Social Change in a Bilingual City. By Marc V. Levine. Philadelphia: Temple University Press. 286p. $39.95.

Can political mobilization and public policy make a difference in the distribution of economic opportunities? In this study of language politics, Marc Levine shows that under some conditions it can. *The Reconquest of Montreal* is a study of how an economically disadvantaged and culturally threatened linguistic community used political action to bring about major social and economic change. Specifically, Levine recounts how Quebecois nationalism provided a channel through which a modernizing middle class realigned the balance of group power between Montreal's francophone and anglophone populations. Working through the provincial government of Quebec, a new Francophone middle class altered language policy, restructured education, and launched a multipronged effort to expand economic opportunity for the province's French-speaking population. Language was centrally important because Montreal possessed a linguistically segmented labor market in which opportunities, especially at the top, were more widely available to those who were English-speaking.

By showing how a number of factors weakened old arrangements and left the way open to create new ones, Levine is attentive to the complicated interplay between politics and markets. The old arrangements were what Levine calls an elite-managed entente (p. 30), and they illustrate how economic power can be used to provide protection for a wide array of institutions and the privileges they afford. The new arrangements show what, under the right circumstances, can be done through an electorally based use of state power. At the same time, Levine is mindful of markets and other forces that limit the capacity of human agents to bring about intended results.

Political scientists and economists, especially those who have a weakness for tidy, monocausal explanations, can learn much from historian Marc Levine. Though his theorizing is not fully explicit, Levine has a keen appreciation of major issues in political economy and a proper skepticism about simple explanations. Furthermore, without ever stating an overt claim, Levine makes a strong case for history as a method for doing policy study. He appreciates both the limits and the possibilities of policy intervention, and he appreciates both *because* of the historical method.

What is the historical method? It is not simply providing a descriptive narrative account. Instead, it is an attempt to explain significant outcomes; in Levine's case, the outcome examined is a largely successful move by a new urban-based francophone middle class to become, in their words, *maîtres chez nous* (masters in our own house, p. 40). The explanation Levine offers is not atheoretical, it is informed by an appreciation of a variety of factors: the fungibility of economic power, relations of exclusion-resistance, consociational accommodation, the embeddedness of consciousness in culture and situation, the dynamics of group mobilization, how market processes promote unintended effects, and much more.

History is about examining confluences, the flowing together of forces—including the purposive actions of change-minded actors. By contrast, much social science attempts to explain by controlling, in laboratory-like fashion, potentially confounding conditions and by *isolating* a cause-and-effect relationship. In examining an uncontrolled world, historians have only their theoretical acuity and their openness to the unanticipated to guide them. What they lose through a capacity for controlled replication, they gain in forced confrontation with the full complexity of real human affairs. Significant outcomes, such as "the reconquest of Montreal" studied by Levine, result from confluences among several factors, not from neatly compartmentalized and prepackaged sets of variables.

As practiced by Marc Levine, history examines a set of arrangements; considers the ways in which various processes strengthen, modify, or weaken those arrangements; and gives particular attention to the efforts of contending actors to reshape arrangements in line with their evolving aspirations and expectations. To talk about events, as he does, in an organic and interactive way is not mere description. Those who insist that explanation must be rendered in terms of universal statements about conditions that can be replicated timelessly confine themselves to the purely mechanistic. Levine's analysis is not so confined and is surely closer to policy reality.

In Kuhnian fashion, one might say that the difference between historical analysis and "normal" social science is not over systematic reliance on evidence but over type of explanation. The point is not to embrace the one best form of explanation but to appreciate the special contribution of each. The complexly contingent character of any generalization is something that history teaches perhaps far better than any other social science, especially when done by an able scholar like Marc Levine.

History does not entail a disregard for general tendencies in behavior; but by focusing on the relationship between confluence and outcome, it provides a check against disembodied explanation. As Levine presents his analysis, he suggests easily a dozen cautions about generalizing from the Montreal experience. And that is an accomplishment that should be appreciated and respected by social scientists of all approaches.

Levine's book is far richer than what can be captured in a short review. It is thoroughly researched, gracefully written, and carefully thought out. It is recommended to students of political economy, ethnic and minority relations, Canadian politics, and urban affairs—

indeed, to all those who have an appreciation for intelligent and sophisticated analysis.

CLARENCE N. STONE

University of Maryland

State and Peasant in Contemporary China: The Political Economy of Village Government. By Jean C. Oi. Berkeley: University of California Press, 1989. 287p. $35.00.

Combining documentary research, refugee interviewing, a return to her ancestral village and on-the-spot interviewing in various parts of China, Jean Oi offers a data-rich and theory-informed comprehension of peasant-state relations as they shape village-level politics, especially the interactions of ordinary villagers with local power holders. Oi enters into a dialogue with moral economists, the most profound researchers of rural China, and analysts of political choice. She concludes that the form of politics during China's imperial era, republican period, Maoist rule, and the present post-Mao reforms remained traditional, that is, personalistic, based on clientalistic relations. Oi locates the cause of the traditionalistic continuity not in culture but in structural circumstances, the similar dysfunctions of different political systems each and all incapable of making a full break into modernity.

Rather than speculating about Chayanov's theories on the economic efficiency of peasant households or dwelling on democratic ideas that link Leninism with traditional Russian feudalism (and thus slighting regional particularism), Oi presents and dissects dramatic data that buttress her powerful portrayal of local-level politics in China. This is strong, first-rank professional work. Her research is so probing that even the best-informed specialist on rural Chinese politics cannot help but learn an enormous amount from every fact-filled chapter of this truly thoughtful and penetrating study. Indeed, the book is so lucid that it cannot but illuminate political reality for any scholar of the vicissitudes of village government or the conundrums of Leninist command economies. The volume should be of great interest to a diverse set of scholars and students.

Oi's focus is the room for maneuver available to villagers as they struggle with the local agents of despotic states over shares of the harvest. In that struggle, markets, sidelines, and secondary labor enhance the independence of villagers and weaken their demeaning dependency on Leninist patrons. Oi also shows how different state policies shape different kinds of local political interaction, theorizing that a Leninist system caught between monopolizing power by maximizing control of the economy and permitting some play to the market and individual mobility only moves China from premodern clientelism to premodern pervasive corruptions of a crony type. Both articulate interest in dyadic modes; but the latter, Oi finds, tends to delegitimate the regime massively even as it increases peasant productivity and village wealth. The ruling groups of the Leninist dictatorship are thus, she persuasively argues, caught on the horns of a painfully wounding, seemingly insoluble dilemma. If they continue the reforms, they will be politically undermined; if they halt halfway, they will be morally delegitimated; and if they retract some of the reforms in order to again strengthen their command of the resources of power (a major tendency in China, Oi notes, since 1984–85), they are back to square one, that is, running a system that cannot deliver the blessings of modernity to the nation, perpetuating the disaster inherited by China's villagers from the Mao era.

Throughout *State and Peasant in Contemporary China,* Oi compares the Maoist state with the Stalinist one in the Soviet Union. She argues that the peasant base of Mao's revolution precluded the direct war on the peasantry that Stalin's urban-based Bolsheviks launched. But Mao's state still wanted the same surplus for similar nonpeasant purposes as did Stalin's state. The result of China's particular Leninist history was a felt need to use only indirect means to gain control of that rural surplus, an imperative, Oi speculates, that forced Mao into what was presented as self-reliant decentralization but actually, as in Stalinism, seized the village's surplus and put it in the hands of local agents of Mao's regime (rather than central agents, as occurred under Stalinist rule). The difference in form made for a crucial difference in local interest articulation but still left the peasantry devoid of means for even relative prosperity.

With real brilliance and extraordinary knowledge gained in a magnificent investigation, Oi explores in exquisite detail all the little pieces that together constitute the rural political economy. She thereby rips away the deceptive veil of taxes, prices, shares, retained funds, and the like (the rhetorical legerdemain of supposedly propeasant Maoism) to reveal that Stalin's cruel political game of taking the surplus from the peasantry and giving little in return was in essence also Mao's game: "Larger harvests did not necessarily mean that peasants received more grain for their labor. . . . Some peasants saw no cash income . . . during the late 1960s and most of the 1970s" (pp. 33, 37). Oi goes on to show how the particulars of the Maoist political shell game of deceiving and defrauding China's peasantry structured a peculiar form of clientelism, one in which villagers found space and means to act on their interests and, in fact, do far better than theorists of totalitarianism would have one believe. But that better turns out still to be quite dismal.

The uniqueness of Oi's contribution lies in the clarity of her simultaneous double focus, capturing both the real content of the Leninist state's disastrous impositions on China's peasantry and the political-social wisdom of China's peasantry in learning how and where and when they could play clientelist politics to enhance the prospects of their living at least somewhat in harmony with the values they cherished. This finely honed scholarly work is informed by rich research, shrewd theory, and mature wisdom. Its dense and complex arguments move at once on so many levels, from Chinese particulars through universal generalities, that the volume, covering 1955–1986, with meticulous detailed attention to the numerous particularities of village political economy, merits close and careful reading, and then rereading.

EDWARD FRIEDMAN

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POLITICAL THEORY

Skepticism and Political Participation. By Aryeh Botwinick. Philadelphia: Temple University Press, 1990. 264p. $34.95.

Political theorists rarely investigate such philosophical underpinnings as epistemology. That is exactly what Aryeh Botwinick does. His is an important exploration of the political implications of a radical epistemological skepticism, earning a place for itself at the center of contemporary discourse. At the theoretical level skepticism seems to lead to a tautological position, undermining itself. But a shift from the theoretical to the practical level can be significant; that is, it becomes collective political discourse in action.

The debate on the possible transformations of contemporary thought centers in many ways on the dichotomy between modernism and postmodernism, between foundationalism and situationalism; in other words, between transcendentalism and "games." It is important to note, however, that for the author, parallels are to be found in this new controversy between the ancients and the moderns. The case is developed throughout the book, but it really culminates in the discussion of the possible convergences between Maimonides and Lyotard. Is the postmodern condition a new argument or really the culmination of the modern project? The argument here is that postmodernism "might constitute the most consummate expression of the modernist program."

The appeal to Maimonides rests on his discussion of God, who as a transcendental cannot be known, since he is beyond human cognition. But the argument leads to an inescapable circularity: in order for God to exist he must certainly not exist. Thus, the transcendental must be contextualized; and here we find the opening for Lyotard's postmodern condition via, of course Wittgenstein. The question then becomes, What can political action be at "the end of history"? Botwinick's answer is that it must be a multiplicity of "language games" in which the rules always change, while the emphasis remains on the pragmatics of the individual game that is being played.

The grand tradition continued today by Habermas is certainly unsatisfied with such a solution. But in the pragmatics of the contemporary world, one must certainly raise such questions. One might even raise the question whether the liberal, as well as the Marxist, projects for a political philosophy may not have exhausted themselves so that the final dialectics of these orientations must be placed at a critical level and rethought. Is pragmatism a real answer? Or is the appeal to pragmatic resolutions the real case? The hermeneutics of Gadamer raised the question of merging horizons all too well; but one can be too interested in merging horizons, as also, for that matter, in playing games. Some issues and people are not for redemption, pragmatically or otherwise. At this point, I myself, although allured by the postmodern case, feel distressed by a simple appeal to localized games. One might, in fact, feel that it is still quite hard to let everything go and risk throwing out the baby with the bathwater.

FABIO B. DASILVA

University of Notre Dame

The Restoration of Political Science and the Crisis of Modernity. By Barry Cooper. Studies in Social and Political Theory, vol. 6. Lewiston, NY: Edward Mellen, 1989. 308p. $69.95.

Canadian Barry Cooper, a lucid and thoughtful writer, chips away at an enormous task: thinking clearly in the face of the disturbing realities of modernity so that political science may be restored to "its full amplitude as a science of human being and history" (p. viii). Such ambitions leave a writer a lot of ground to cover; for Cooper's notion seems to be that thinking well about how the world has changed means thinking first about how others have thought. In 15 short essays Cooper gives us his wisdom on a host of intellectual problems (the fact-value distinction, the impact of technology on politics, the difference between leadership and statesmanship), historical moods and movements (nihilism, phenomenology, hermeneutics), and important writers (his favorites are Arendt, Strauss, and Vogelin, with Vogelin getting his own chapter). He sets a dizzying pace without a tone of apology. An essayist, he says, should push an argument to its limit, with "a minimum of qualification or second thought," marching forward in "a mood of considerable speculative confidence." The reader should be warned, Cooper suggests, that the goal is illumination, not new information, and that the argument will be "highly allusive" (p. 155).

Cooper is least satisfactory at his most allusive. Often what we get is commentary upon commentary. We do not get Cooper on hermeneutics, but Cooper on whether Zygum Bauman does justice to the relationship between hermeneutics and social science (he doesn't, really). The point of his account of phenomenology is to render a judgment about whether Hwa-jol Jung understands Husserl (he does, pretty well). Cooper is more engaging when he is straightforward: telling an audience at the Center for the Study of Theory and Criticism in Ontario to study their Plato and Aristotle, trotting out Mathew Arnold as a social critic worth taking seriously, or praising Eric Vogelin for an empirical analysis of the world that attends to the problem of consciousness.

What is rare in these pages is a sustained presentation of Cooper's own views. Cooper is not shy; but the matter is hard and the "crisis" distant enough that one can afford to rush through the books of others without rushing to final conclusions of one's own. While the "three great political scientists of the century" (p. viii) urgently confronted the crisis of modernity in its "particularly virulent form" in Nazi Germany, Cooper confesses to a tamer background and less urgency. Canada's past is not as nonviolent as some people think, but it is a civil country even in the midst of constitutional crisis. And for political scientists in Canada "modernity took the much less dangerous form of intellectual confusion" (p. ix). It is triumph enough to make headway and to avoid muddleheadedness—the muddleheadedness, for example, of behavioral political scientists or political theorists who take a history-of-ideas approach to their work. (David Easton and George Sabine are "brothers under the skin," Cooper says, "intellectual stepsons of Hume, as it were" [p. 13]).

Cooper's essays will not hold much interest for a general readership. They will be particularly off-putting to two sorts of readers: those who care more for politics than for theory will lose their way among the allusions and look in vain for connections to concrete political life (or almost in vain: the chapter "Statesmanship and Leadership" has useful things to say about Churchill and Thatcher, Woodrow Wilson and Mackenzie King), and those who believe that political theory is pointless unless it fairly directly confronts the need for prescription and action will dislike Cooper's abstractions and the academic quality of his discussion. Cooper should find an audience, however, among those who share his preoccupation with the muddied currents of abstract political philosophy in the twentieth century. When the currents are muddy, it is no small thing to swim toward clear water.

BLAINE GARVIN

Gonzaga University

Rights: The New Quarrel between the Ancients and the Moderns. By Luc Ferry. Translated by Franklin Philip. Chicago: University of Chicago Press, 1990. 151p. $21.00.

Despite the title, Ferry does not provide much examination of rights. Part 1 of this book analyzes Leo Strauss's view of modernity and critique of German idealism; part 2 explains Fichte's critique of rationalist historicism and theory of freedom and rights. Ferry intends the result of these two examinations to require logically (and promote in practice) a modern, rights-based political philosophy. He further intends to establish a new view of modernity by showing that modern thought need not be historicist. Ferry concludes, as opposed to Strauss's view, that the real quarrel between the ancients and the moderns is not a choice between teleological versus nonteleological thought but rather a choice between dismissing rights and freedom or defending them as fundamental.

Ferry says that Strauss believes the preeminence of modern subjectivity produces an individual's loss of reference to a transcendent standard and, having no ultimate purpose, people are thrust into absolute historicism. Ferry regards Strauss's resultant return to the ancients as entirely inappropriate: the political motivation of Strauss is seen as "deeply antidemocratic" and "inegalitarian" (p. 20). Ferry mistakenly sees critics of modernity such as Strauss as "resolutely unable . . . to think of something like human rights" (p. 21). Though Strauss regards modernity as having collapsed into historicism, Ferry argues that some thinkers are modern without becoming historicist. Ferry claims that "none" of Strauss's criticisms of modernity apply to Fichte (p. 69), because Fichte does not agree with other moderns that the rational equals the real and because Fichte's major purpose was to attack such a view.

Ferry rightly notes that Strauss's critique of Rousseau is extended by Strauss "to all philosophies of freedom" and therefore "to all of German idealism" (p. 45). Ferry questions whether Strauss has recognized that "Rousseau's philosophy begins with a reasoned critique of the first wave of modernity" (p. 39). (Strauss did recognize it). And Ferry does not know how we should understand Strauss's claim that Rousseau's philosophy led to a more radical form of modernity and therefore to radical historicism (p. 39). Ferry's failure to understand and describe this aspect of Strauss's analysis causes Ferry to overlook serious consideration of Strauss's claim about the nonteleological aspect of much of modern thought.

Strauss objected to Rousseau's philosophy of freedom and therefore to German idealism on the grounds that they can lead to a Hegelian coincidence of the real and the rational, the *is* and the *ought* and that they make the goodness of an action depend on the mere generality of human will without reference to the requirements of human nature and excellence (pp. 47–48). Regarding the first objection, Ferry cogently argues that Strauss's periodization of the history of political philosophy "glosses over the tensions and diversity" of the second wave (p. 51) and that dogmatic development in the second wave of modernity was a betrayal of the early modernists. However, Ferry lets the second (and more important) objection drop without trying to join the issue.

Ferry's problematic understanding of philosophy is illustrated by his reference to the "deeply nonhumanist" (p. 50) quality of classical political philosophy—oblivious to the central role of ancient Greek philosophy in the humanist tradition of Western civilization. Ferry neglects both philosophical investigation of the nature of humanity and attention to major exponents of humanism in Western civilization.

Strauss's return to the ancients presents a serious intellectual dilemma for him, argues Ferry: if actualization of the best regime depends on chance, "shouldn't we irrevocably forgo all actions meant to actualize the ideal?" (p. 57). Doesn't Strauss's denunciation of Machiavellian realism presuppose the will to "conquer chance" and "to have some effect on reality" (p. 57)?

Though Ferry does not claim to make an original contribution to the understanding of Fichte, Ferry does intend to recommend strongly the interpretation of Alexis Philonenko's *La liberté humaine dans la philosophie de Fichte* (1966). Ferry argues that political philosophy becomes thinkable and still remains modern because it manages (through Fichte) to show the descent into historicism was *not* inevitable. As Ferry is aware, Strauss did not write much on German idealism. Ferry regards discussion of this issue as an appropriate way to promote greater appreciation of Fichte.

Though Ferry's work is more useful as a critique of Strauss than as an exposition of Fichte and/or German idealism, its contribution to our understanding of Strauss is modest and the writing is abstruse. Kenneth Deutsch and Walter Soffer's edited *Crisis of Liberal Democracy: A Straussian Perspective* (1987) is uneven but more useful for understanding Strauss. A very comprehensive and provocative, though seriously flawed, account of Strauss, is Shadia Drury's *Political Ideas of Leo Strauss* (1988). While Drury has a natural law perspective, Ferry champions individual rights without a grounding in natural law. Ferry lays out Strauss's objections to German idealism but does not seriously join the issue with Strauss regarding the difference between the ancients and the moderns.

GORDON J. TOLLE

South Dakota State University

Democratic Individuality. By Alan Gilbert. New York: Cambridge University Press, 1990. 510p. $55.00 cloth, $18.95 paper.

One of the oldest problems of ethics and political theory is how to establish fundamental principles of moral conduct to guide and evaluate the activities of individuals, groups, organizations, and governments. At one time, moral standards were thought to be revealed to us by God or based on our intuitions. But one person's God turned out to be another's devil, while your and my intuition of the good need not be identical. The most popular contemporary theories eventually reduce to moral relativism: in utilitarian theories any one preference is as good as any other (as Jeremy Bentham said, "Pushpin is as good as poetry") and contractarian theories (e.g., John Rawls's) that promise objectivity threaten to become relativist by reducing to cultural and societally dependent value consensus. Is there an alternative? Can moral standards be established in some more rigorous, objective, even scientific manner?

In an audacious argument, Alan Gilbert's *Democratic Individuality* argues that there are such standards. He argues that without them, we would never be able to condemn murder, rape, and savagely repressive regimes like that which existed in Nazi Germany. The core moral standard for judging all human activity is based on mutual recognition of, and regard for, persons, a socially based and dependent—or in Gilbert's words a democratic—individualism. He argues that all persons have the possibility for moral personality, that this is the objective basis for our valuing preservation of life and the possibility of a good life. Gilbert also argues that there is nothing ethnocentric about this core notion.

Gilbert's view of moral objectivity is tied to his notion of moral progress. He argues that our objective understanding of moral values progresses—much the same way as human understanding of the physical world progresses —toward greater and greater approximation to the truth (with, of course, occasional setbacks). This view he calls *moral realism;* it bears strong, explicit parallels to the theory of scientific realism defended by many post-positivist philosophers of science, including Richard Boyd, Dudley Shapere, and the early Hilary Putnam. Many current philosophers of science argue that the scientific theories that we consider true are the ones, in comparison with other theories, that have the most explanatory power. Some philosophers of science (e.g., Feyerabend and Kuhn to a degree) seem to conclude that scientific knowledge is subjective, bearing little or no relation to a fixed empirical reality; scientific theories cannot thus be compared across epochs. In contrast, scientific realists hold that these theories are not just better than their competitors but also true; they bear progressively closer relation to a real empirical world "out there." Scientific realists argue that scientific advance is progressive, that successful theories are approximately closer to the truth than their predecessors. Gilbert argues that the gaining of moral knowledge is similar, that moral learning is progressive, that moral objects are objective, really out there (hence *moral realism*).

The progress of moral understanding, according to Gilbert, proceeds historically. Alfred North Whitehead once asserted, "All western philosophy is but a footnote to Plato." But he could not have had ethics and political theory clearly in mind. Gilbert, like many of us, begins with Aristotle. He argues that moral objectivity is displayed in Athenian society, that the place to begin understanding ethics and social and political life is with Aristotle's notion of social individuality. While Aristotle's view is a great advance, it is defective in that he does not extend his understanding of human personality to include slaves, barbarians, or women. Historical experience, including slave revolts such as that led by Spartacus, teaches us that all humans must be accorded their rights. Similar advances have been made with respect to recognition of the full humanity of women. Insights and key formulations (i.e., expansions and enrichment of the original Aristotelean premises) have been provided by Montesquieu, Mill, Kant, Hegel, Marx, and modern feminists, among others. The new advances and enhancements of the notion of human personality have been made possible, according to Gilbert, by factual discoveries displayed throughout the development of human history.

Gilbert's strong position, its sharp contrast with the most prominent alternative theories today, including those of John Rawls, Leo Strauss, and the utilitarians, and his willingness and readiness to take on all challengers will undoubtedly make it an important pole in debate and may well turn *Democratic Individuality* into a classic.

The presentation of this argument, however, is only a small part of the book. In a running series of arguments, critiques, asides, and often lengthy digressions, Gilbert gives analyses and criticisms of literally hundreds of past and living theorists from Plato, Locke, and Hobbes to Strauss, Kripke, G. A. Cohen, and Fishkin. Although his willingness to take on all comers often makes the book difficult to follow, the insights are often pithy, even brilliant at times. The two theorists dealt with in the greatest length are, not unexpectedly, Weber and Marx.

One of the most fascinating parts of the book is Gilbert's lengthy reinterpretation of Weber's corpus. He is presented as a highly contradictory thinker with shifting standards and sometimes penetrating insights. *The Protestant Ethic and the Spirit of Capitalism,* contrary to standard interpretation, is seen as the work most compatible with Marx's analysis of the origins of capitalism. Gilbert also traces Weber's many antiliberal aspects, particularly in his later work. Gilbert examines Weber's ambivalence toward eugenics and his adherence to highly vulgar features of German nationalism. All this makes fascinating and compelling reading. Less compelling is Gilbert's attempt to portray Marx as a highly voluntarist theorist, whose political prescriptions are quite divorced from the level of economic development in a society and even from a consideration of many economic interests. Thus, his sharp disagreements with Trotsky seem especially surprising, since the latter is also often accused of having an overly voluntarist conception. While rightly critical of interpretations of Marx that are overly economic determinist, Gilbert seems to bend the stick much too far in the other direction.

Also somewhat unusual is the attempt to minimize the differences between sophisticated liberal and radical (and Marxist) positions. The argument in the book posits adherence to similar moral standards by both perspectives. The differences are over such "empirical" matters as to whether capitalism can be reformed and its politics made to be democratic or whether socialism is feasible and can be controlled from below. While the discussion of this theme is intriguing, it is not argued in a compelling and systematic manner.

Although it is easy to be picky and find disagreements, *Democratic Individuality* is a tour de force, dealing with literally hundreds of vexed questions of political and social theory. The brief critiques of Strauss and Hayek, for example, in my opinion, hit the nail on the head in an illuminating manner. The discussions about the capitalist pressures on U.S. university life and the antidemocratic nature of antiradical and anti-Marxist biases among Anglo-American social scientists are lucid and poignant. When Gilbert applies his theory of democratic individuality to questions of euthanasia and the right to die, the discussion is not only striking, but by far the best I have seen.

Democratic Individuality is a treasure chest, a book destined to play an important role. It is not, however, easy to read. Its many interwoven themes, critiques, diversions, even shorthand remarks, do not merely make it difficult to follow at times but require the investment of considerable effort to digest completely. Nevertheless, the effort will be richly rewarded.

MICHAEL GOLDFIELD

Cornell University

When Knowledge Is Power: Three Models of Change in International Organizations. By Ernst B. Haas. Berkeley: University of California Press, 1990. 274p. $29.95.

International organizations, as Ernst Haas observes in this thought-provoking but sometimes difficult book, frequently experience change in the definition of the problems they are expected to solve, in other words, in their "problem sets." But how, when, and why do such changes take place? What can we say about their consequences? To answer these questions, Haas identifies and analyzes three analytically distinct patterns of change (or, as he describes them, *models* of change): adaptation through incremental growth, adaptation through turbulent nongrowth, and learning to manage interdependence.

Adaptation involves efforts to improve links between means and ends "without questioning the theory of causation defining the organization's task" (p. 37), whereas learning is marked by a reassessment of "the basic beliefs underlying the selection of ends" (p. 37). Incremental growth and turbulent nongrowth are different settings that combine with adaptation to produce distinct models of organizational development (p. 4). Incremental growth occurs when an organization takes on new tasks without changing its decision-making procedures. Turbulent nongrowth is a condition marked by a loss of coherence and an erosion of internal consensus on both means and ends.

Changes in an international organization's problem set emerge from bargaining processes that are influenced by forces exogenous to the organization itself (p. 27). But what factors drive these bargaining processes? More specifically, what factors determine whether they will eventuate in one or another type of adaptation or in learning? Several irritating features of this book make it difficult to tease out and evaluate Haas's answer to this question. Definitions of key concepts are not precise enough to allow the reader to apply them with confidence to actual cases. How, for example, can we measure when and to what extent an organization's problem set has changed? Though Haas is laudably modest about his contributions to knowledge ("A typology is not a theory," p. 6), he immerses us in a sea of conceptual distinctions and taxonomic exercises that make it difficult to grasp clearly and to hold onto the central themes running through his analysis. While he clearly writes from a neopositivist vantage point, the fact that Haas eschews sustained empirical analysis in this book (in sharp contrast to his earlier works, *The Uniting of Europe* and *Beyond the Nation State*) increases the problem confronting the reader who desires to make a judgment about the validity or the persuasiveness of various claims advanced in the book concerning the forces at work in bargaining processes affecting problem sets and the extent to which these changes conform to one or another of his three ideal types.

Yet it is important not to allow these problems to blind us to the fundamental contribution of this book. Students of international politics have long sought to explain the outcomes of bargaining processes in terms of the roles of power (in the sense of the possession of material resources) and bargaining skill. Without in any way rejecting the relevance of these factors, Haas directs our attention repeatedly to the role of knowledge as an independent force affecting the course of human affairs. As he puts it, "change in human aspirations and human institutions over long periods is caused by the way knowledge about nature and about society is married to political interests and objectives" (p. 11).

The application of this train of thought to the study of change in the problem sets of international organizations takes two principal forms. One highlights the role of "consensual knowledge" in determining whether such changes qualify as learning or are confined to adaptation. At the end of a discussion containing numerous insights regarding the role of consensual knowledge, Haas offers the strong proposition that for learning to occur "there must be sufficient consensual knowledge available to provide the rationale for the novel nesting of problems and solutions" (p. 164). The other turns on the role of epistemic communities—groups of professionals who share both a system of causal beliefs and a set of political values—not only as forces in the emergence of consensual knowledge but also as agents in the use of this knowledge to redefine the problem sets of international organizations. While this discussion does little to resolve the controversy over the identification of epistemic communities in empirical terms and leaves many questions about the interactions among epistemic communities unanswered, it does suggest the proposition that these communities are "the most significant agents of institutional change" (p. 40), at least when it comes to alterations in the problem sets of international organizations.

Whatever its shortcomings, this book deserves a careful reading on the part of serious students of international institutions. Whether we are better off in the long run treating knowledge as a form of power or as a determinant of change in international affairs distinct from power in the material sense, it is undeniable that we need a much improved grasp of the role of ideas at the international level. Ernst Haas has been making this case for a number of years. Despite its limitations, this book constitutes the most sustained expression of the case we have, at least when it comes to the analysis of international organizations and institutions (to his credit, Haas correctly and clearly differentiates the two). We should therefore treat it not as a flawed summation of a completed stream of analysis but rather as a point of departure for a growing body of innovative thought.

ORAN R. YOUNG

Dartmouth College

Calvin and the Foundations of Modern Politics. By Ralph C. Hancock. Ithaca: Cornell University Press, 1989. 221p. $26.50.

In this study Ralph Hancock interprets and attempts to locate Calvin and Calvinism in the history of political thought. In part 1, Hancock explores topics directly related to politics, and in part 2 he explores Calvin's "antitheology" of "transcendence without another world."

Part 1 concludes with Hancock's thesis: "Calvin radically distinguishes the divine and the human [worlds], precisely for the purpose of joining them together more powerfully than before." Calvin distinguishes, but does not separate, these worlds because he recognizes "only one world and . . . it is all God's" (p. 119). Calvin's justification by faith alone, unlike Luther's, frees the believer "from preoccupation with the inward condition of his soul" (p. 132), thus transforming "Christian inwardness into outward activity" (p. 141). This permits the view that natural sensual desires are good or innocent and the conclusion that reason (the cause of the fall) need not order the desires (pp. 146–48).

For modern politics this thesis means there is no necessary opposition between revelation and rationality. It justifies as providential the pursuit of objectives, unrestrained by human authority, that both manifest God's glory and are radically confined to this world (p. 118). Calvinism is "worldly prosperity" bound in an "inner dynamic" with "religious fervor" (pp. 118, 208).

For modern political philosophy this thesis means neither world "forms an intelligible whole for human beings" and that the opposition of the two worlds is for the purpose of rejecting the classical philosophical understanding of the human soul as essentially rational and hierarchical (pp. 119, 148). Hancock's thesis fully explains Troeltsch's discovery of the contradiction within Calvin's theology of the alliance between natural law rationalism and divine will irrationalism. Calvin's teaching here coincides with Hobbes's: "The impotence of reason with regard to purposes must be established as a condition for the rationalization of human society" (p. 114).

Hancock establishes Calvin's political and philosophic *conclusions* as conventionally modern respecting acquisitiveness, rationalism, and overcoming nature. But Calvin's *influence* is accounted for by his providing, more clearly than modernity's recognized philosophic founders, the "nonrational idealism" or "spirit" that is inextricably bound up with, and necessary to the success of, "the rational materialism of the modern age" (p. 21). The idealistic impulse to worldly economic and political activity, and self-transcendence, produces "a full prosperity" (p. 116) to the benefit of both "God's innocent natural creation but especially to the glory of God" (p. 166). Hancock thus finds mistaken both Skinner's view that "a providentialist argument must be foreign to a secularist argument" (p. 118, cf. 185-186) and Strauss's closely related view that "no religious body of thought can be accounted a direct source of modern rationalism" (p. 21).

Hancock deepens Weber's view that capitalist modernity is *original* Calvin, against the view of Doumergue, Strauss, and Tawney that modernity is *secularized* Calvin. The deepening is this: Modernity can get capitalism from an entirely this-worldly Lockean acquisitiveness. What modernity cannot get from Locke—and what Calvin provides—is an impetus to a noncapitalist modernity that would remake the world in what it might call a "humanitarian" direction. The modernity-as-secularization thesis rejects glorifying God as an incentive for such politically and economically useful virtues as hard work, self-discipline, delayed gratification, and law-abidingness. Hancock's account does not suggest whether secularized (i.e., allegedly purely rational) modernity can explain, preserve, and justify *these* virtues. It does suggest that secularized modernity cannot give a consistent definition of itself. One wonders, in particular, why secularized modernity should prefer a humanitarianism that resembles Christian love to Nietzsche or to Lenin's isms? On the other hand, if "humanitarian" modernity requires Calvin's reliance on a power beyond reason, that also means that modernity cannot give a fully rational definition of itself. Modern rationalism, if it wishes to be humanitarian, seems decisively to depend on the Christian revelation from which it thinks itself emancipated.

This book brings to the fore the question of the source and nature of the spirit which moves modernity. Above all, it is a meditation on the relation between reason and revelation that explores the possibility that original modernity may bind them together inextricably, symbiotically, and synergistically. The radicalness of this possibility for the self-understanding of modernity and of religion within modernity can scarcely be understated. Upon gazing into the seemingly dissolving distinctions between reason and revelation and between secular and religious, one would barely hazard the opinion that incipient modernity absorbed Calvinism by becoming dominated by it. Thereafter, two possibilities—and therewith two kinds of modernity—can and do follow: a secularized modernity that gives up Christian revelation and its accompanying "spirit" and a modernity that preserves that revelation and that spirit in mutually reinforcing tension with purely philosophic elements. The possibility of a heterogeneous modernity that somehow includes revelation as an alternative to a homogeneous secular modernity excluding revelation is nowhere more plausibly suggested than in this book.

GARY D. GLENN

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Happy Slaves: A Critique of Consent Theory. By Don Herzog. Chicago: University of Chicago Press, 1989. 308p. $45.00 cloth, $12.95 paper.

The Authority of the State. By Leslie Green. Oxford: Clarendon, 1988. 283p. $49.95.

Each of these two books is, of its kind, excellent; and their kinds are closely connected, for each is a scrutiny of "consent theory," the doctrine that the state's authority rests, and can legitimately only rest, on the consent of the governed. But Herzog's account is historical, high-spirited, and beguiling while Green's is analytical, painstaking, and artless, its persuasiveness resting entirely on the high quality of its argument. Both are well worth the reader's time, even if readers who admire Green's meticulousness may find Herzog a touch too reckless for their tastes and admirers of Herzog's way with seventeenth-century political controversy may find Green's analytical jurisprudence a shade too sober.

Herzog's intellectual tactics are professedly opportunistic: he mixes historical, philosophical, and literary analysis as he needs them. In his index *Norman Yoke* jostles *Nozick, Robert,* and he explores the consent theory at the heart of the modern liberal theory of the state through the astonishing things that Hobbes in particular (but other late Tudor and Stuart writers, too) managed to do with the concept of consent three-and-a-half centuries ago. His tactics aren't hit-and-run, however. The heart of the book is a sustained account of sixteenth- and seventeenth-century anxieties about the rise and prevalence of "masterless men," based on substantial reading in the controversial literature of the time and the work of historians such as J. H. Hexter, Christopher Hill, Conrad Russell, and Lawrence Stone. Within that framework, he provides a dazzling account of Hobbes's *Leviathan,* demonstrating at point after point how Hobbes employs the doctrine of consent to bring us under the state's authority and prevent our escaping it. Herzog is analytically ruthless in unpicking Hobbes's contradictions and contextually sympathetic in showing how they are forced on him by his need to give masterless men a reliable master. Those of us who think Hobbes is a better philosopher than that won't agree with the portrait in every detail, but it is extremely well done.

If the apparatus is, to this extent, historical, the polemical point of the book is contemporary. Don Herzog ties the doctrine that political obligation must rest on consent to the notion of liberal neutrality put forward by Bruce Ackermann and Ronald Dworkin—rightly, in the sense that consent-based obligations are essentially "content-neutral": the fact of a promise having been made imposes the obligation, not the content of the promise. On the other hand, he does not think that the appeal to neutrality really picks up the main point of consent theory, which he argues by producing an entertaining variation on one of Bruce Ackermann's fictitious dialogues. The bare notion of neutrality cannot pick out principles of social justice or constitutional choice that are acceptable to liberals from ones that are not. To do that job, we have to resort to a substantial conception of the social universe, the separation of church and state, and the inviolability of the individual of the kind that Locke's *Letter on Toleration* offers. The title of the book expresses Herzog's sense that we are less aware than was Locke of the real extent of our masterless condition and the wholesale contingency of our political condition. To the extent that we see our current social, political, and economic arrangements as natural and inescapable, we are the happy slaves of his title; the point of the doctrine of consent is to remind us of the choices we might have made and did not.

Leslie Green proceeds in a cooler and steadier fashion but to astonishingly similar conclusions. He gives an exemplary account of the place of political obligation among other obligations and an equally exemplary account of the place of obligation based on the authority of the state among all the other reasons for obedience to law that we might have. For instance, I ought to obey the law that forbids me to murder you, just because I should not murder you in the first place; but this obligation is a moral one and has nothing to do with the state's authority over me. The bulk of the book is devoted to showing that once we pick out the relationship at stake (that the state is entitled to issue commands to its citizens that bind them regardless of their content, that its commands provide what Joseph Raz baptised "exclusionary reasons" for obedience), we discover that it is hard to justify that relationship. We can uncover innumerable other relationships, but they chronically fail to add up to *authority.*

Among the pleasures of the book is a careful account of "the self-image of the state" that separates out the notion of political authority in a defter fashion than any account I know of. Another is an elegant discussion of the thesis that individualistic theories of obligation are bound to fail whereas communitarian accounts show how we are, so to speak, permeated by the authority of the community whose values constitute our selves. Green argues that this kind of argument will not do, because it does not distinguish between communities that rightly and justifiably exercise authority over us and those that do not. Only consent will do, even if the consenting individual is more "situated" than the asocial inhabitant of Hobbes's state of nature. One way and another, it is on the "individualist and critical spirit of consent theory" that the emphasis falls. Like Herzog, Green rates obedience a lesser virtue than civility; and while agreeing that a state that secures the consent of its subjects is to that degree superior to a state that does not, he is clear that political order must rest on a good deal more than authority—on the state's effectiveness in securing justice, above all. It is somehow rather reassuring that Green's cautious analytical approach and Herzog's exuberant historicism should converge so neatly on this point.

ALAN RYAN

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Taking Liberties: National Barriers to the Free Flow of Ideas. By Elizabeth Hull. New York: Praeger, 1990. 173p. $38.95.

Of all the liberties we as a people revere, none are more jealously guarded and defended in the courts and in the public mind than the First Amendment liberties dealing with freedom of speech. Yet the extent to which U.S. policy should encourage First Amendment values in matters that extend beyond our borders has long presented challenges to commentators and policy makers. A tension between our ideals and reality exists in this area. On the one hand, there is the belief prevalent in domestic political affairs that information and ideas should be freely available for consideration and debate by all, competing for consideration and credence in a "marketplace of ideas." As the distinguished Judge Learned Hand observed, the First Amendment "presupposes that right conclusions are more likely to be gathered out of a multitude of tongues, than through any kind of authoritative selecting. To many this is, and always will be folly; but we have staked upon it our all" (p. 109). Yet numerous regulatory obstacles prevent the flow of information across United States borders, thereby undermining the notion of competing ideas so central to our concept of freedom of speech.

Elizabeth Hull's *Taking Liberties* analyzes the effect on First Amendment values of a wide range of restrictions on cross-border information flows. Hull especially criticizes the McCarran-Walter Act of 1952, which serves as the basis for excluding controversial foreigners from visiting the United States and which became law despite President Truman's veto and his observation at the time that "seldom has a bill exhibited the distrust evidenced here for citizens and aliens alike" (p. 15). The Internal Security Act, the Foreign Agents Registration Act, and various export control laws are also scrutinized here. As Hull notes, voices of dissent from official policy further the public debate on critical issues, whether the view is voiced by a citizen or a foreigner. However, imposing restrictions on foreigners rarely generates much concern in the government because of the lack of political representation. Hull suggests that public officials do a disservice to the values central to an informed and involved citizenry by shortsightedly curtailing access to vital information from whatever source, particularly in an increasingly interconnected and interdependent world. In many ways, invoking "national security" concerns (the alleged basis for many of the restrictions discussed here) has become a mere talisman, "used to legitimate all sorts of governmental restrictions, from the necessary to the unnecessary to the indefensible" (p. 155).

A recurring theme of the book is the degree to which certain phenomena in our political system contribute to this undermining of First Amendment values. Hull criticizes the Supreme Court's extreme acquiescence—via the "political question doctrine" and other judicial devices—in executive branch decision making related to foreign matters. Discussing the Court's deference to the executive office's interpretation of a particular measure intended to restrict travel to Cuba despite a clear congressional mandate to the contrary, Hull perceptively suggests that the Court's deference to the political branches in this, as in so many other matters, is essentially deference to only one branch, the executive. The analysis of the role of the judicial branch at both the Supreme Court and lower court levels is well done throughout this work.

Another systemic phenomenon affecting this area is the substantial discretion wielded by the nonelected administrative officials who implement the myriad regulations. Under many of the regulations, these officials' only guidance as to who or what should or should not be admitted, excluded, or encouraged in the interest of national security is their own sense of patriotism and political ideals. This is hardly a secure basis for invigorating public awareness and debate. Hull's account of political and administrative regulation of information flow is further testimony to the ever-increasing rise of the politically unaccountable "administrative state."

Hull notes at the beginning of her book that many of the regulatory obstacles she discusses originated in the Cold War era, and that the easing of East-West tensions may eliminate many of the policies most deserving of criticism. Many of the policies, directed toward the preservation of national security in the face of a perceived ominous Soviet menace, undoubtedly will fall. The work is extremely well documented throughout, and Hull does not lose sight of critical concerns where there may be legitimate reasons to curtail informational flow. But with the United States now facing severe economic competition from a variety of its trading partners, it is unlikely that some of these policies (e.g., those dealing with technology transfer) will be necessarily or quickly dismantled. Nor is it obvious that such policies should be entirely eliminated. Her discussion in this area is too abbreviated, given that Hull is not suggesting an absolutist First Amendment position. Her analysis of possible approaches to evaluating and balancing the competing considerations consistent with a principled policy to furthering First Amendment values could therefore be fuller. However, on the whole, Hull's analysis and conclusions are worthy of much consideration.

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Heidegger, Art, and Politics. By Philippe Lacoue-Labarthe. Translated by Chris Turner. Cambridge: Basil Blackwell, 1990. 141p. $43.95 cloth, $17.95 paper.

Heidegger and Modernity. By Luc Ferry and Alain Renaut. Translated by Franklin Philip. Chicago: University of Chicago Press, 1990. 130p. $16.95.

On the basis of a consensus that includes such disparate thinkers as Leo Strauss and Jacques Derrida, Martin Heidegger was the greatest thinker of the twentieth century. Unfortunately, assessing the ramifications of his thought has been complicated by a diversion of energies occasioned by another round of discussion of the infamous "Heidegger affair." The latest round was initiated by the recent publication of Victor Farias' *Heidegger and Nazism*. Given the philosophical merit of the book, it is hard to understand the great stir that Farias has caused, spreading even into the popular press. The book contains hardly any revelations that are new, shows little indication of serious reading of Heidegger's texts, and develops much of its case against Heidegger through guilt by association.

No doubt the reason for the stir caused by Farias' book must be found in the political and intellectual environment in which it emerged. With the political collapse of Marxist communism, one is forced to wonder whether Marxist and neo-Marxist thought will not inevitably be tarred with the same brush. If so, Heidegger's philosophy could conceivably provide the basis for an alternate corpus of thought. Because of a concern that that might happen, the political battle has been enjoined even if it has no bearing whatsoever on the philosophical merits of Heidegger's thought. Those who cannot confront the ideas of an important thinker attack his character instead. But seminal ideas cannot be dismissed in this easy fashion; by their very power they will undermine other ideas and thereby have ramifications. But despite the disappointing nature of the Farias volume, it has occasioned a lively debate, with the present two volumes being among the more useful and serious entries.

The facts surrounding the "Heidegger affair" are straightforward, have long been known, and are in little dispute. In 1933 Heidegger joined the Nazi party and became rector of

Freiberg University. In less than a year he resigned as rector. Disappointingly, Heidegger never simply repudiated his 1933 decision and added to concern by still talking about "the inner truth and greatness of national socialism" as late as 1957. He engaged in a series of compromising actions during the 1930s, behaved questionably and/or in a cowardly fashion at the death of his teacher Husserl, and in an infamous postwar observation, equated the extermination of the Jews with mechanized modern agriculture and the forced deportations of Germans from Silesia.

There is little question that Heidegger exercised poor judgment, behaved in a thoroughly ungentlemanly fashion on a number of occasions, and was resolutely and unnecessarily obstinant in his refusal to denounce the concentration camps and reflect on his practical miscalculations. The question is, What bearing does this have on Heidegger's thought? Is it true, as Adorno argued, that Heidegger's thought is infected to the very core with fascism? A second question follows: Even if it were so infected, is it possible to simply reject a corpus of thought as powerful as Heidegger's and assume that thereby it can be denied any influence? Or do ideas have a power of their own that cannot be consciously negated in this straightforward fashion? If so, it is always necessary to come to grips primarily with a thinker's ideas and their intrinsic ramifications.

Lacoue-Labarthe argues that Adorno's understanding of the fascist infection of Heidegger's thought is mistaken. Nonetheless, he concludes that Heidegger's decision in 1933 was no mistake. He also argues that Heidegger consciously and resolutely opposed the central theoretical presuppositions of modern democratic political and moral life: rationalism, subjectivism, individualism, cosmopolitanism, internationalism, the ideas of progress and enlightenment, and so on. This stance left Heidegger open to fascism, which inarticulately shared the same terrain. Heidegger further believed that everything great and noble requires rootedness in a distinct people. But for Heidegger, a people is formed not biologically but linguistically. Each people represents an individual linguistic whole whose language reveals the world in a unique way. As such, it is not possible to translate easily from language to language and people to people.

Lacoue-Labarthe argues, therefore, that despite Heidegger's dismissal of democratic domestic politics and internationalist world politics, he clearly rejected the biologism of national socialism. Heidegger likewise rejected the possibility of the consciously projected politicization of science attempted by national socialist ideology. Lacoue-Labarthe concludes that while Heidegger's thought opened him to elements of national socialist ideology, it also allowed him to see where it was defective. For Heidegger, the actual national socialists were at best revisionists. In his own eyes only he, Heidegger, represented the "inner truth and greatness of national socialism." Finally, Lacoue-Labarthe goes on to argue that the basis of "Heidegger's politics" is to be found most clearly after the famous break or "turn," especially in the discussions of poetry and art. We will return to this issue in a moment.

Ferry and Renaut provide an enlightening discussion of the emerging orthodoxy regarding the Heideggerian "turn." They argue that the preoccupation with Heidegger's alleged change of heart after 1934 is primarily a political move on the part of a generation of Heideggerians bent on completely exonerating him. An orthodoxy has emerged that, in the authors' terminology, pits Heidegger I against Heidegger II. It is argued that Heidegger I is a humanist and metaphysician caught in the morass of voluntarist and subjectivist terminology, the error that leads Heidegger I to his flirtation with Nazism. Heidegger II rejects Heidegger I and moves to a new nonmetaphysical, nonhumanist mode of thought. Heidegger II is in no way a Nazi. Hence, Heidegger's Nazism is dismissed as an error occasioned by his mistaken early thought; it is a sign of the unfinished nature of his thought, not its deepest intent. His fateful decision is, therefore, in no way related to his true, fundamental understanding. Ferry and Renaut try to show why one must reject this political gambit. In the process they try to argue for the necessity of a return to modern humanism. They attribute this orthodox position to Lacoue-Labarthe as well. But while Lacoue-Labarthe thinks Heidegger's politics can be most easily discerned in the later works, he holds a position not unlike Ferry and Renaut's. There is a continuity in Heidegger's thought from beginning to end, and the Nazi experience is not an accident.

Lacoue-Labarthe sees Heidegger as the latest proponent of the understanding that politics is a form of art. Politics does not have a natural basis (as, for example, Aristotle maintained). The political does not balance and weave together natural givens but is creative and constitutive. Lacoue-Labarthe designates this position as the "aestheticization of politics" (pp. 62–76). The city is seen as constituted by works of art. Since for Heidegger language takes priority among the arts, poetry takes primacy in the constituting of the city. According to Lacoue-Labarthe, Heidegger was on the way to this understanding from the beginning. This accounts for why the later Heidegger turns to discussions of poetry and art. Even the discussions of technology aim at showing how it blocks the possibility of genuine poetry. What differentiates the early from the late Heidegger is the increasing clarity that true constituting acts are unconscious. Great art is not a conscious act, it is "granted" to the artist by an act of grace that emanates from some mysterious source. By working through language, the poets constitute a people as a distinct language community. In this way a people constitutes and continually reconstitutes its essence as its work, but never self-consciously.

Lacoue-Labarthe sympathizes with this position, as well as with Heidegger's attack on the allegedly metaphysical Western tradition. Regarding the aestheticization of politics, Lacoue-Labarthe admits that Heidegger is not alone on this path; in fact, he states that Heidegger represents a kind of romanticism (p. 53). And as in romanticism, Heidegger links his understanding with the need to recover the original Greek view that only a myth allows a people to accede to their history. Like Heidegger, Lacoue-Labarthe opposes modernity and its reliance on subjectivism, rationalism, and so on and sees modernity as the cause of our troubles, as well as the necessary outcome of the Western tradition.

Ferry and Renaut, however, try to position themselves within a revitalized modernity by attempting to show the intrinsic totalitarian limitations of Heidegger's thought. They see modernity as a form of humanism that reaches its highest manifestation in the tradition of Rousseau, Kant, Sartre, and Adorno. That tradition aims at constructing a doctrine of freedom where freedom is seen as requiring the absence of any definable code, whether natural or traditional. Freedom requires that an individual be understood as a self-defining nothing. Premoderns believed in a predefined natural code. Hence, they had no doctrine of freedom. Heidegger, Ferry and Renaut argue, ultimately falls back (in an allegedly neoconservative fashion) on a reliance on closed traditions. Ferry and Renaut argue that only with a modern doctrine of freedom is democratic politics possible; for democracy presupposes a notion of an individual as an actor who makes conscious choices, and this in turn allegedly requires a doctrine of an individual as a nothing.

Ferry and Renaut further argue that when one builds on the notion that an individual is a nothing, one realizes that individuals are forced beyond their particularity toward the abstract universal. In other words, individuals understand the necessity of a cosmopolitan definition of their fundamental humanity. According to Ferry and Renaut, the refusal to accept this cosmopolitanism leads inevitably to fascism, as the example of Heidegger allegedly shows. It is Heidegger's antimodernity that forces him into the arms of national socialism.

Interestingly, Ferry and Renaut admit that when Heidegger talks of an individual as *ek-sistent*, this means that one has no subsistent naturalness defining one's being. Indeed following Lacoue-Labarthe's argument that Heidegger's politics are based on the notion of politics as art, we would conclude that Heidegger, too, believes that we are nothings who must constitute ourselves. But Heidegger, in opposition to Kant, Sartre, and Adorno—but probably not Rousseau—does not think this can be done consciously. He concludes that this self-constitution is accomplished only unconsciously by closed groups, so that hence our fundamental nothingness produces a result that is inevitably particularist rather than cosmopolitan.

Heidegger attempted to show that the subjectivism upon which Ferry and Renaut try to build leads inevitably to technological domination. Ferry and Renaut argue that subjectivism is needed for democratic politics. Who is to say whether Heidegger or Ferry and Renaut have better understood the logic of presenting a human being as a nothing? A more modest question: Is it possible to criticize modernity without becoming a fascist?

Ferry and Renaut admit that their position, which requires that human beings must be wrenched from both nature and tradition, poses the problem of limitlessness. We are potentially confronted with the groundless will to will. It is this limitlessness that led Heidegger to conclude that rootedness in a distinct people was the only alternative available in the absence of nature. Heidegger may be wrong, but we miss the point if, like Ferry and Renaut, we view him as a neoconservative determined to impose tradition upon people by force (pp. 74, 79). Unless Ferry and Renaut's critical, modern theory can find some way to circumvent the limitlessness they clearly see, Heidegger's response becomes more attractive, perhaps inevitable. Or perhaps one must reconsider the modern rejection of nature that Heidegger, Lacoue-Labarthe, and Ferry and Renaut share. Must one be a nothing to be a decent human being?

With the assistance of Heidegger, both of these works lead us toward the most important and fundamental questions confronting contemporary thought. If the "Farias affair" has helped initiate this serious debate, it will have proved valuable—but primarily as something quickly to transcend. Both volumes help lead us beyond the dismal rehashing of the Heidegger affair toward serious questioning. It is on that higher ground that we should conduct our confrontation with the thought of Heidegger.

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The Political Theory of Liberation Theology: Toward a Reconvergence of Social Values and Social Science. By John R. Pottenger. Albany: State University of New York Press, 1989. 264p. $44.50 cloth, $16.95 paper.

Liberation Theology and Its Critics: Toward an Assessment. By Arthur F. McGovern. Maryknoll, NY: Orbis, 1989. 281p. $14.95 paper.

These are two provocative studies of the political thought associated with the Latin American Christian radical movement known as liberation theology. The Pottenger volume appears to be a revised doctoral thesis and argues that the liberation theologians offer a "unique" way to bridge the gap between fact and value that has characterized social science since the Enlightenment, claiming "to provide a comprehensive alternative to the politics of mainstream religious thought, to redefine the ethical parameters of political activism, including the use of violence, and to close the historic rift between orthodox Marxism and mainstream Christianity" (p. 3). McGovern, the Jesuit author of *Marxism: an American Christian Perspective* (1980) evaluates both the theological and sociopolitical elements in the liberation theologians' writings in the light of the substantial critical literature about them that has appeared in recent years. Both authors address the issues of Marxism, violence, and the class struggle as well as the place of the Christian Base Communities in liberationist thought. Of the two books, McGovern's is more comprehensive, up to date, readable, and (with some exceptions) convincing.

Pottenger argues that liberation theology is "a genuine philosophical movement whose political theory, if fully developed, has the potential . . . to speak to the limitations of traditional ethical arguments on political behavior, and to contribute significantly to contemporary debates on epistemology" (p. 189). Yet the actual treatment of the substance of the liberationists' thought is limited to English language sources and analyzes only a few of the many writers on liberation theology. Pottenger does not show how they represent a philosophical movement, an ethic, or an epistemology, as promised. He also admits that the liberation theologians lack a systematic analysis of the relation of reform and revolution, have no theory of just social revolution, and do not understand the role of the market.

Pottenger discusses the possibility of using critical theory of the Marcuse-Habermas variety as a basis for a critique of contemporary poverty and injustice; but there is little to justify his conclusion that liberation theology offers a comprehensive alternative to other theories. The Christian Base Communities, the most socially significant expression of contemporary liberation theology, get a very brief treatment that concludes that "their transformation of society through reform efforts may be only marginal at best" (p. 141). Tell that to Lula, the Labor party candidate for the presidency of Brazil, who came very close to winning in the December 1989 runoff, or to the new woman mayor of Sao Paolo—both with close ties to the Base Community movement.

Pottenger devotes a chapter to the relation of Marxism and Christianity; but it is little more than a summary of Marx's views on religion and some mild criticism of liberation theology's opponents. A much more interesting discussion appears in the McGovern book. He argues that "if one looks at the bulk of the writings by liberation theologians, Marxism plays a relatively small part" (p. 145). McGovern admits that discussions of Marxism were common in the 1960s and 1970s, but he asserts that it was not central to their thinking at any point (p. 230). Yet he is aware that a common element in the liberationist critique is anticapitalism, the basis of that anticapitalism being the belief that Latin America's problems are ultimately rooted, as Gustavo Gutierrez first stated in the basic text of the movement, *Towards a Theology of Liberation,* in "the private ownership of the means of production" (p. 202). Why is private property the problem and socialism the solution? Because of the acceptance of Marxist theories of exploitation and class struggle. Implicitly or explicitly a fundamentally conflictual ("dialectical") view of society based on Marx leads the liberation theologians to a "preferential option for revolution" as the solution. Simply to cite their criticisms of some aspects of Marxism, as McGovern does, does not refute those who argue that Marxism is indeed central to the liberationist critique of capitalism as the source of oppression and exploitation in Latin America.

Similarly, McGovern tends to downplay the fascination of some liberation theologians, particularly in the 1960s and early 1970s, with the possibilities of violent revolution as an instrument of social change. He states that "although I have tried to search out writings that deal with the question of violence, I have yet to find any statement by a liberation theologian calling for violent revolution" (p. 186). Yet he follows this with two widely quoted statements by Gutierrez that come very close to supporting revolutionary "counterviolence." In addition, there are the late Ignacio Ellacuria's statements in the early 1970s that violence "performed on behalf of the oppressed" is "good violence" and that "in cases of established [i.e., institutionalized] violence, whatever form it may take, we may be not only permitted but even required to use the force that is necessary to redeem the established violence."

Apart, however, from his excessively apologetic attitude on the issues of Marxism and violence, this is a very useful book. It combines theological sophistication, historical knowledge, personal interviews from two trips to Latin America, and a knowledge of the latest writing in Spanish and Portuguese to give a comprehensive evaluation. It differs from most treatments in the emphasis it gives to feminist themes and to the importance of the new ecclesiology (what McGovern calls "a new way of being church") that is represented by the base communities in Brazil and Central America. It is fair and evenhanded in its treatment of critics like Michael Novak and Cardinal Ratzinger; and it criticizes the sacred cows of liberation theology when they deserve to be criticized, for instance, Leonardo Boff's claim after a 1987 visit to the Soviet Union that it was a "clean healthy society" that inspired his group to think that the kingdom of God was being realized (p. 183).

McGovern ends the book asking whether for all the enthusiasm about the movement, "doing creative theology from one basic paradigm (challenging and deideologizing older interpretations of Christianity and gaining new insights from the perspective of the poor) may reach a limit" (p. 232). He concludes (as so many of the rest of us have) that it is time for more discussion of specific ways in which the poor can become effective agents for social change. That, of course, is a central issue for the Left in a newly redemocratized Latin America where the revolutionary rhetoric of the period when liberation theology was born now sounds quaintly obsolete and Gustavo Gutierrez has discovered to his discomfort that in Peru the principal agent of social transformation through revolutionary violence is the Sendero Luminoso.

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Justice and Modern Moral Philosophy. By Jeffrey Reiman. New Haven: Yale University Press, 1990. 336p. $30.00.

The search for political and moral "truth" has been subjected to sustained and sometimes effective attack. Various writers have undermined the quest for metaphysical "foundations" for political institutions. They find such foundational justifications unnecessary and self-defeating. Instead, they defend their political ideals on practical grounds. Rawls, for example, argues that his results hold only for societies with liberal democratic traditions and explicitly limits his discussion to the conceptions of a modern constitutional democracy.

Reiman believes that this shift toward a more pragmatic form of justification is a mistake. He argues that political philosophy needs foundations if it is to adequately answer the "suspicion of subjugation," that is, the suspicion that its proposed ideals are actually disguised mechanisms for dominating others. In *Justice and Modern Moral Philosophy* he attempts to answer this suspicion by providing "principles of natural and social justice that are binding on all rational individuals" (p. ix).

Reiman has written a very thoughtful book that presents a foundational basis for social and political norms. And while I do not find his defense of deriving norms from certain "facts" of human nature compelling, I think that he raises some important substantive problems for contemporary political theory. In particular, his arguments underscore the inadequacy of liberal theory's treatment of power and its failure to recognize the special nature of human labor.

Reiman's is a naturalistic theory. He begins with a thick conception of the person (in contrast to Rawls's thin theory) that is meant to be true of persons as rational agents, and therefore true across all cultures. According to this thick conception, which is "essentially Heideggerean," (p. 15), all human beings confront death and know that they do so (at least after some age). This knowledge lends an urgency to the fulfillment of human projects.

The list of these projects is itself uncircumscribable. As with contemporary liberalism, Reiman locates judgments of value in the individual; only the individual can judge the ultimate values around which he or she builds a life. Yet despite the diversity of values and projects, all human beings are "naturally" equal in "living their lives as their one and only chance in eternity to live the life they want to live" (p. 49).

Based on this naturalistic fact about persons, Reiman argues that all persons have an interest in not being subjugated. For whatever their particular projects, in order to be able to attempt to achieve them, individuals must be secure against subjugation. Thus, subjugation becomes the key lever at work in justifying social and political norms and institutions. Just institutions must not be subjugating. Here, Reiman raises an important worry for liberal theory: perhaps contemporary norms and values are the product of subjugation; for example, perhaps there is consensus around these values only because the voices of "marginal groups" have been excluded. We cannot simply take these norms and values as *given* when justifying political ideals. Reiman believes that the weakness of liberal political theory, exemplified in the work of John Rawls, is its reliance on norms that have not been defended adequately.

Like Reiman, I think that accepted norms are not merely by that fact acceptable. Unlike him, I think reasons in favor of many of these norms can be given that do not depend on showing that these norms are "requirements" or "derivations" of rationality. Political justification can also rest on what Rawls refers to as "ideas of reasonableness," widely shared normative ideas that are implicit in the public culture of a democratic society. If such ideas (e.g., respect for other persons, fairness) are widely accepted on the basis of critical reflection, open discussion, and historical experience, why should we worry about whether or not they are "true" requirements of rationality? Ideas of reasonableness may be as widely shared as ideals of rationality and thus serve equally well as a basis for political argument.

Of course, not everyone is reasonable, and certain cultures reject values of equality and respect. But is everyone and every culture "rational" in Reiman's sense? This brings me to Reiman's actual theory, which argues that the human interest in avoiding subjugation is a requirement of rationality, binding on all individuals including those who do not share liberalism's traditions and aspirations. I am skeptical of this argument for two reasons. First, subjugation is a complex social fact. As Reiman recognizes, much subjugation is invisible, embedded in social structures and relations. How are we to identify it? Is there even consensus on what constitutes subjugation and what is only justified interference with the welfare of others? Does subjugation mean the same thing to Foucault and a Dervish mystic?

While the various world religions have given markedly different answers to the meaning of death, Reiman attributes to it a singular meaning. Second, I doubt that subjugation is a "natural kind" term, referring to the same relationship in different contexts. Subjugation exists in both the workplace and in the home, but its mechanisms are very different. Reiman offers no analysis of these differences.

Yet one can drop Reiman's metaphysical justification of political norms and institutions and still retain many of his substantive conclusions. Reiman argues that whether or not people cooperate to produce benefits, such norms as noninterference, easy rescue, respect for natural ownership, trustworthiness, and intergenerational solicitude should govern their actions. Where they do cooperate to produce benefits, they owe each other distribution of "the benefits and efforts that went into producing them" according to Rawls's difference principle (pp. 297ff.).

Reiman's primary contribution, I think, is not methodological but substantive: it lies in his explicit attention to considerations of power, particularly the power located in unequal distributions of human labor. For the human capacity to labor (what Marx calls labor power) is not simply another good to be distributed like housing or groceries. How we regulate exchanges of human labor has a profound influence on the human beings whose labor is exchanged; for in this case, the parties to the exchange are themselves constituted by the exchange process. The exchange of human labor under conditions of unequal ownership gives rise to domination and subjugation, to dominating and subjugated individuals. Liberal theory has by and large ignored the distinction between distributions of human labor and other types of goods. By contrast, Reiman places this distinction at the center of his discussions about justice. He argues that when we think of economic distributions as distributions of human labor between individuals, the relation between distribution and subjugation is made evident. Unequal distribution gives some individuals control over the labor of others. And such control subverts the conditions in which individuals can be equally free from subjugation. But the undesirability of subjugation, to reiterate my point, is not a "requirement of reason" but a conclusion of claims and aspirations found within our own society. Nevertheless, anyone concerned about power and subjugation needs to think about the special nature of human labor and about Reiman's attempt to integrate this fact with liberal political theory.

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The Masks of Proteus: Canadian Reflections on the State. By Philip Resnick. Buffalo: McGill-Queen's University Press, 1990. 340p. $24.95 cloth, $9.95 paper.

One thread weaving its way throughout this very readable and thought-provoking interdisciplinary work is Resnick's contention that the state in countries as diverse as capitalist, liberal-democratic Canada and the socialist, centralist Soviet Union could benefit from much more devolution of powers and popular sovereignty (direct and participatory democracy) à la Rousseau rather than parliamentary supremacy (indirect and representative democracy) à la Edmund Burke. For the author, such an orientation is relevant to attempts to comprehend the fundamental causes of, and possible solutions to, the 1990 Meech Lake constitutional fiasco in Canada. It is also useful for analyzing reasons for, and alternatives to, the rapid disintegration of failed top-down command models of bureaucratic socialism in societies once organized on the Leninist basis of democratic centralism. The author is a well-respected member of the Canadian Left who has contributed significantly in previous writings to a better understanding of Quebecois nationalism and the inextricable links between socialism and the national question in Canada. Publication of this book comes at a time when English Canadian intellectuals like Resnick are grappling with the potentially lethal impact on Canada's distinctive national identity stemming from both the Canada-United States Free Trade Agreement (1988) and the post-Meech Lake resurgence of political sovereignty with or without economic association variants of Quebecois ethnonationalism.

Resnick calls for abandonment of the search for some grand theory of the state based solely on one particular discipline or methodological approach because "the state wears many masks—coercive and consensual, centralized and decentralized, democratic and anti-democratic, economic and social, sovereign and dependent" (p. 5). The malleable character of the state presumed by the author alludes to "the masks of Proteus," a god-figure in Greek mythology known for frequent changes in shape and appearance. Hence, the reader is presented with a wide-ranging yet well-organized inquiry into the highly diverse and constantly changing explanatory theories regarding the origins, nature and scope, functions, and evolution of the modern state in Canada compared mostly to other First World (especially U.S.) and Second World (especially Soviet and East European) societies.

Part 1 draws on the history of Western political philosophy, which suggests to Resnick a fivefold typology of theories of the state: aristocratic, republican, "order," liberal, and democratic (p. 13). He then proceeds to trace how the concepts and terms derived from political theory enter into the direct practices of societies like the United Kingdom and the United States compared to Canada. Part 2 relies on the insights of political economy and political sociology to construct a primarily thematic and descriptive overview of the mushrooming modern literature on functions of the state. Social scientists' emphasis on this theme during the Reagan decade parallels a growing acknowledgment by neo-Marxists, social democrats, and Keynesian liberals (as well as neoliberals and neoconservatives), each for different ideological reasons, of the crucial importance the state has acquired in response to "economic crisis, war, and societal development" (p. 132). Resnick then selectively borrows from these sets of ideas to improve his understanding of the enormous expansion of the role of the Canadian state in chapters on "organized capitalism," the limits of a Royal Commission, and Canada in the capitalist world economy. This portion of the book concludes with an explanation of the significance of the qualitative shift in Canada's relative economic influence compared to other state actors in the international system over the past 20 years from semiperipheral to being one of seven leading capitalist powers of the world—that is, Canada is now at the perimeter of the core. As a result, Canada can no longer limit its horizons to North America alone or to its bilateral relationship with the United States. She now has a moral and political responsibility for the shape of the capitalist world economy (p. 204).

Part 3 explores the distinctively divergent roles played by the state and the nation in the development of English Canada and Quebec, how it came about that most socialist thinkers and doers in Canada and elsewhere have historically opposed federalism and the largely negative implications of this socialist rejection of decentralization. The book ends with a plea for a Jacobin-Rousseauean-citizen Tom Paine reconciliation of socialism, democracy (including federalism), and the state. After all, *vox populi vox dei est,* in heaven and on earth—whether in Canada or elsewhere, whether socialist or otherwise.

In conclusion, Resnick has intelligently synthesized and distilled from a clear and consistent critical radical liberal (popular participatory) perspective insights on the ever-changing character of the state derived from a wide variety of sources ranging from political theory and political sociology to social democratic theory and praxis in Canada. Thus, he has substantially enhanced the reader's understanding of the role of the Canadian federal state, which has been undergoing drastic transformation in recent decades as it confronts the interrelated challenges of North-South continental integration and counterstate-building Quebecois nationalism. On the other hand, he does not prove conclusively that direct popular participation in formulating the general rules by which "We the People" shall be governed—whether in societies beguiled by the heritage of aristocratic republicanism (divided sovereignty) or tory parliamentarism (legislative paramountcy) or victimized by capricious interpretations of democratic centralism—immunizes those places against the effect of Michels's "iron law of oligarchy."

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Time, Freedom, and the Common Good: An Essay in Public Philosophy. By Charles M. Sherover. Albany: State University of New York Press, 1989. 322p. $59.50 cloth, $19.95 paper.

Charles Sherover's *Time, Freedom, and the Common Good* is an apology for the economic and political institutions of Western countries, which are taken to provide a via media between anarchic libertarianism and centrally planned socialism.

Sherover calls the structure of advanced capitalist countries "democratic mercantilism." He analyzes it from two points of view: historical and phenomenological. He gives an account of the historical development of the theory of representative democracy and the separation of powers from Aristotle on. And he offers a phenomenology of the human condition, articulated around three modes of being in the world: sociality, temporality, and freedom.

Two features of this book deserve praise. The first is the interweaving of U.S. and European contemporary philosophical traditions. Efforts by scholars on this side of the Atlantic to produce a syncretic view are welcome and enriching. Sherover tries to conjugate phenomenology and pragmatism, by emphasizing that democratic pluralism can gain more from a complex rendering of the experience of freedom in human life than from accounts of human motivations based on utilitarian considerations or rational choice theory.

The second positive feature of Sherover's book is his attempt to overcome the by-now-traditional and slightly stale dichotomies pervasive in the philosophical and political debate in the United States. By this I mean the dichotomies between individual rights and the common good, the market and "big government," and "abstract foundationalism" and communitarian contextualism.

This attempt is apparent in Sherover's account of sociality, temporality, and freedom. Sociality indicates that the human species is constituted as gregarious and that we are mutually dependent on one another. But we are also free. Despite scientistic attempts to subject human life to the laws of causation, the *experience* of freedom, says Sherover, is fundamental and undeniable. Only by assuming that we are free can we explain the human capacity for creativity and innovation and the sense of responsibility that we feel when we make decisions for ourselves and others. Temporality both limits and enhances freedom. As individuals located at a given historical juncture in time, we inherit conditions not of our own making. As participants in the activity of defining what a still-open future is going to look like, we exercise our freedom, that is, our power to make something pass from potentiality to actuality or to prevent something from ever coming into being.

At the political level this means, according to Sherover, that we must uphold the basic features of our society, which does not shun state regulation and intervention but deliberately leaves things open, undetermined, unplanned. This economic and political system guarantees freedom of action to the individual without neglecting the care for the common good. The individual depends on his or her fellows and cannot flourish if they do not flourish as well.

Sherover's analysis of freedom, both in personal and political terms, is the focus of his study; it is the most interesting part, but it is also the most problematic. Sherover argues in favor of a public philosophy centered around the phenomenological concept of freedom. He also appears to refer indirectly to the contributions of epistemologists who analyze human actions that cannot be subsumed under the causal model. According to recent epistemology of the social sciences, the Humean model of causation can be employed to account for human agency as much as for events in the natural world. There is no event, including human actions, that does not have a cause. But where does the cause come from? From an antecedent cause? Or are we capable of "creating" out of nothing? Can we be initiators of an undetermined causal series? If we answer in the affirmative, what are the consequences for political philosophy? Should we allow individuals to express it unhindered? What type of polity can we construct for human beings who are partially determined—elements in the chains of causes and effects—and partially "wild cards"?

Although Sherover appears to aim at treating these issues, his position is not always clear. First of all, he seems to identify freedom as "freedom of choice," with freedom as the "capacity to determine the will" (p. 60). The two are not identical: on the contrary, in the philosophical tradition of the West, freedom of choice is compatible with the idea that our will is not free, that we do not create out of nothing, and that all we can do is to choose among existing possibilities. Sherover gives the reader the impression that when we exercise freedom of choice we also exercise the freedom to determine the will. Perhaps the latter can be reduced to the former; but given that the two have traditionally been kept distinct in Western philosophy, the burden of proof falls on the person who wishes to unify them.

This is not merely an analytical point. It has, it seems to me, important consequences for Sherover's public philosophy. In early modern and modern liberalism, freedom of choice is associated with "negative liberty" in political matters. To be free to choose has been associated with the idea that there are options and possibilities open to each individual and that the state is not going to restrict our freedom of choice any more than is strictly necessary. Not surprisingly, Sherover shares this view (p. 8). In his public philosophy, the state is not the minimal state of the libertarians; but it must keep off of each individual as much as possible. The aim of the individual is to subtract him- or herself from the control of the state, so that the possibilities available to him or her will increase. But when that space is guaranteed, the individual will not only exercise freedom of choice but also be the undetermined initiator of causal series (p. 121).

This view is paradoxical from two points of view. First, the aim of human beings who are defined by "sociality" is to get away from it as much as possible (pp. 46, 80, 131, 235). Each person aims at controlling personal time and what to do with it, unhindered by others and by material dependence. Sociality defines us as human, but is a burden on us.

The second paradox is that Sherover does not offer any exhaustive account of the exercise of positive freedom in the public arena. Sherover briefly mentions (pp. 54, 228, 236) that human beings are going to exercise their capacity for bringing the new into the world both in their personal lives and in politics. But these references do not substitute for a thorough analysis of how human beings practice positive freedom when acting in concert. In other words, Sherover's "sociality" never becomes Arendtian "plurality." This is evident in Sherover's account of the development of "democratic mercantilism." Even if we accept his laudatory version of Western history (at times excessively laudatory, given its well-known dark sides), that history does not appear to be the product of free individuals acting in the world but of impersonal forces. One example will suffice. In describing the evolution of capitalism, Sherover writes:

> Whatever criticisms of capitalism's conquest may be valid, a new creative force had *been unleashed.* It should *be harnessed,* . . . not repudiated as Marx demanded. Indeed, during its second hundred years, it accelerated its advance while harnessing itself by the reforms which the democratic capitalist countries all continued to make since the Great Depression of 1929 (pp. 216–17; emphasis mine).

The use of the passive voice here is revealing. Were not those reforms and that harnessing the outcome of struggles and conflict in which individuals, groups, and classes engaged in defending themselves from unjust and oppressive social relations, aiming at shaping the future by translating into reality a vision created by their imagination? Do not human beings experience their creative potential by acting with others? Why should we overwhelmingly desire to be free in solitude and isolation? At a historical juncture when the mortgaging of the future through technological change has become a reality, the issue of acting creatively in concert to keep the future open occupies the center of the political stage. A program aiming to preserve existing institutions, such as Sherover's, appears insufficient to rise to this call even if offered with passion, accuracy, and rich scholarship.

DANIELA GOBETTI

University of Michigan

Liberation Theology at the Crossroads: Democracy or Revolution? By Paul E. Sigmund. New York: Oxford University Press, 1990. 256p. $29.95.

Liberation Theology at the Crossroads: Democracy or Revolution? is essentially an extended review essay of the works of major liberation theologians in Latin America. The author's theme is that the content of liberation theology needs to shift its emphasis from making vague predictions of revolution in Latin America to offering specific suggestions for building participatory, democratic structures in the region.

Sigmund characterizes liberation theology as one of the more enduring products of the late 1960s, singling out the year 1968: "The clearest beginning of the new line of theological speculation is in the writings of Gustavo Gutierrez in the period leading up to and following the 1968 Medellín meeting of the Latin American bishops. What the meeting did was to legitimate a new kind of Catholic radicalism in Latin America that could now cite the official statements of the bishops in support of their arguments" (p. 28).

Those arguments, in brief, were that theology should be a "scripturally-based critical reflection on experience (praxis)" of the poor, using social science tools of dependency theory and Marxist analysis (p. 29). However, Sigmund frequently points out that the early works in liberation theology were especially long on social science and short on Scripture. His book provides a useful survey of works of the most prominent scholars: Gutierrez, Miguez Bonino, Hugo Assman, José Miranda, Juan Luis Segundo, Enrique Dussel, Leonardo and Clodovis Boff, Jon Sobrino, and José Comblin. Comblin receives by far the most favorable treatment by the author because of his consistent opposition to violence and to Marxist socialism.

Sigmund's survey of these scholars and their impact in the Americas is written in a highly personal style due to his not-so-hidden agenda to influence the future development of liberation theology. He waits until the last chapter to summarize his opinions explicitly: "Specifically, I believe that the liberation theologians are wrong in holding (a) that the primary source of oppression is capitalism and of liberation is socialism, (b) that the poor have a superior insight into religious truth, and (c) that liberalism is to be rejected by biblically oriented Christians" (p. 182).

The chapter then elaborates on these points, reiterating his previous observations. However, Sigmund's discussion regarding liberation theology's rejection of liberalism is far too brief, given the complexity of the topic. He uses a string of shorthand labels to identify different strands of liberalism: classical liberals, contemporary liberals, communitarians, modern reformist liberals. The result is a hasty and confusing summary of the major themes of Western political philosophy. As if he senses the confusion, he finally states, "This is not to argue that the liberation theologians should become liberals. . . . It is only to insist that the structural critique should incorporate a recognition of the value of democracy, individual and social rights, and the rule of law" (p. 193).

In his final assessment, Sigmund notes that recent works in liberation theology are now closer to his way of thinking, particularly as they back away from revolutionary Marxism. He wisely includes excerpts from 1970 and 1984 publications by Gutierrez that provide a fascinating example of how this conceptual evolution is occurring. The content of liberation theology is changing in Latin America based on praxis, and this is entirely in keeping with its open-ended, dynamic essence.

One important question the author does not address is the serious challenge to liberation theology posed by the surge in Latin America of the evangelical churches. Religion is inevitably politicized in Latin America, thus the theological polarization of Protestant evangelism versus liberation theology often translates into a political polarization between support for the status quo versus calls for revolution. The strength of the evangelical churches varies from country to country; therefore, the degree of polarization and tension in particular Latin American nations deserves careful study.

Sigmund's personal agenda gives his book an energy and a sense of commitment often missing from scholarly texts, but it also prevents the book from being anything close to a definitive study of liberation theology. Instead, for anyone seeking a balanced perspective, *Liberation Theology at the Crossroads* should be but one title on a list of recent books analyzing this powerful school of thought.

ANNE MOTLEY HALLUM

Stetson University

The Political Meaning of Christianity: An Interpretation. By Glenn Tinder. Baton Rouge: Louisiana State University Press, 1989. 257p. $29.95.

The importance of the study of religion's influence on politics has become, in recent years, part of the conventional wisdom. Glenn Tinder's book, however, takes us a step beyond the empirical search for politically relevant variables to digging into the depths of the meaning of Christianity itself. His book is admittedly "a personal statement" (p. 1) of a believer in God, Christ, and the Trinity, who stands in the tradition of the Reformation. The Reformation viewpoint he affirms is that of Martin Luther, and his "position involves an emphasis on human fallenness that many Christians are apt to consider excessive" (p. 2).

The political meaning of Christianity, Tinder contends, is that believers should take a prophetic stance (being in the world but not of it) on the side of the exalted individual (God gives humans glory that we can freely affirm). This stance is bolstered by a prophetic hope (based on an understanding of our fallenness and eschatological role) that leads us to defend liberty (in the context of community, with a spirit of tolerance, in opposition to power) and to work for social transformation (to remove obstacles to community and push toward our destiny). This hope is sustained by a prophetic spirituality (adopting a listening and waiting attitude that helps us discern when to obey, when to disobey, when to stand silently, and when to act hesitantly or regretfully as it may be).

Tinder's book is written from within a strong tradition, with all of the richness of depth, pitfalls, and blind spots that implies. The book presents a clear reflection on the implications of Martin Luther for the twentieth century. It posits an answer on how and why someone should act in a fallen world.

According to Tinder, humans and the world are indeed fallen: "People are deeply and persistently selfish" (p. 5); ". . . all are persistently and deeply inclined toward evil. All are sinful" (p. 36). "Society . . . arises ultimately from human sin and is shaped by sin throughout" (p. 54); "Evil is rooted in the nature of human beings and not in the means they employ" (p. 135). "Politics is a realm of moral darkness, and the darkness cannot be dissipated by human virtue and wisdom" (p. 145).

I find this negative view of humanity, the world, and politics profoundly disturbing. Although Tinder claims to be elaborating on the political meaning of Christianity, he seems to have neglected the Resurrection. I have no doubt that there is evil in the world. The fact that in the United States approximately one out of three females and one out of six males are sexually abused before the age of eighteen is sufficiently dramatic to dissuade one of a totally naive optimism. However, at times, his theology seems to be pre-Christian rather than Christian. His is a theology of the Fall of humanity and of the crucifixion, rather than of the redemption of humanity through the Resurrection and the showering of grace through Pentecost. He describes the human condition in this way: "A human being, as known to us in the world, is a creature of darkness, filled with darkness in the form of sin, moving toward the darkness of death, and hedged about always by the darkness of finitude" (pp. 225–26). This is not a description of a member of an "Easter people."

His interpretation of Christianity provides challenges both to the self-righteous, intolerant Christian Right and to the occasionally Polyannaish Christian Left: "Liberty has to be defended, even though it brings the injustice the left feels to be intolerable and the unrighteousness the right would suppress by law" (p. 114). Social transformation comes about because Christian principles place one in a critical relationship to established institutions: "The Kingdom of God is a judgment on the existing society" (p. 154). Yet because humans are fallen they cannot expect to create a perfectly just society: "Our proper aim is the elimination of particular injustices, not the creation of perfect justice" (p. 159).

Tinder's critique of liberation theology is consistent with his doctrine of the Fall of humanity; however, it is based on a rather superficial reading of that school of theology. Some of his own positions seem consistent with those who see the message of Christ as one of liberation, for instance, "The faith that God has given every individual a worth beyond human measurement is socially explosive. It does not merely condemn manifest injustices. It questions all inequalities of wealth, rank, and power" (pp. 165–66); "The respect owed every human being is grounded in divine *agape* and calls for opposition to every institution that treats human beings neglectfully and oppressively" (p. 168); "In prayerful reflection, one tries to attain an understanding of history and the Kingdom of God and to speak and act appropriately in response" (pp. 200–201). Tinder interprets the prophetic stance as one that requires prayerful waiting in preparation for acting and acting in attentiveness to God and humility to his direction (p. 218).

One of Tinder's favorite theologians appears to be Reinhold Niebuhr, who has been attributed as the author of the often cited "serenity prayer":

God grant me the serenity
To accept the things I cannot change,
Courage to change the things I can,
And wisdom to know the difference.

The basic difference between Tinder's Lutheran approach to the political meaning of Christianity and the rest of the Christian tradition (fifteen hundred years of experience prior to Luther and the overwhelming majority of Christians since then who are not Lutheran) is that many of the rest believe that God, because of Redemption, can grant them the courage (and the grace) to change many more things than Tinder believes possible. Therein lies the difference between a politics of the Fall and a politics of the Resurrection.

THOMAS J. HOFFMAN

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AMERICAN POLITICS

Keeping a Watchful Eye: The Politics of Congressional Oversight. By Joel D. Aberbach. Washington: Brookings Institution, 1990. 288p. $28.95.

For years scholars, including Morris Ogul and Seymour Scher, have argued that members of Congress have little incentive to engage in systematic, comprehensive oversight of the bureaucracy. The little oversight that they do is sporadic and often ineffective, given members' incentives to protect the programs under their committees' jurisdictions. More recently, others have argued that while congressional committees do not engage in comprehensive, self-initiated oversight, they nevertheless do pursue frequent and effective oversight in response to external indications (from constituents, interest groups, or the press) that something is amiss in the programs under their jurisdiction, an approach that Mathew McCubbins and Thomas Schwartz have called "fire alarm" oversight. Joel Aberbach similarly argues that Congress has become an energetic overseer of the executive branch; but he takes issue with McCubbins and Schwartz's argument that congressional incentives are conducive to fire alarm oversight but not to "police patrol" oversight, which involves self-initiated, systematic sampling of agency behavior and outputs to determine compliance with congressional intent.

Tracing the incidence of oversight hearings from 1961 to 1984, Aberbach documents a dramatic increase in this type of oversight activity beginning in the early 1970s and continuing through the early 1980s. From extensive interviews primarily with the top staff members of eleven committees, he shows how oversight issues get on committees' and subcommittees' agendas and the techniques by which oversight is conducted. Committees employ a variety of informal, as well as formal, ways of collecting information about the programs within their jurisdictions, creating an intelligence system in which both staff and members are alert to indications of trouble from many sources within and outside government.

Aberbach argues that the increase in oversight in the 1970s resulted from a changed congressional incentive structure. Oversight once offered little electoral benefit, but in the early 1970s the payoffs increased. The new incentives include a variant of the familiar electoral one: constituents, frustrated by an increasingly complex government, responded more favorably than before to Congress's attempts to correct problems in executive branch agencies. This incentive was reinforced by growing budget constraints, which made creation of new programs difficult, and by challenges from the Nixon administration, which sought to exert greater control over policies and programs. Thus, Congress increasingly used oversight to control and protect programs that benefited their constituents and key clientele groups. At the same time that the payoffs of oversight increased, so did Congress's capacity to engage in it, by means of the decentralization of power to subcommittees and increases in staff size during the early 1970s.

The argument that these changes increased the payoffs of fire alarm oversight is a plausible one. But Aberbach goes further, arguing that Congress also performs more self-initiated oversight as it engages in active search for information without outside stimulus. There is no apparent incentive, however, as Aberbach and many others have shown, for committees actively to seek out problems in programs that members largely support when there is no indication that executive political appointees have interfered and clientele groups and constituents are apparently satisfied. Moreover, the evidence from the interviews for this claim, although intriguing, is not developed enough to be fully convincing, since it does not spell out completely the activities that comprise active monitoring or make the case that those activities are not inspired by outside actors.

Aberbach delves deeply into the techniques by which congressional committees and subcommittees gather information about agencies and programs. However, unlike Christopher Foreman (*Signals from the Hill* [1988]), he does not examine directly how Congress sends signals to executive agencies about their own policy preferences. Some of the oversight techniques that he describes (such as member communications with the executive branch and the legislative veto) can be vehicles for those signals, while others appear strictly to be information-gathering devices (such as examination of agency reports). Thus, it is unclear how Congress uses the information it gathers during oversight to enforce its will.

There are numerous tables throughout, the most useful of which are the contingency tables of interview results. In addition, however, several chapters are based on regression and factor analyses, the purposes of which in a number of cases are unclear. For example, chapter 6 contains a factor analysis of oversight techniques, but the results are not used to test hypotheses or even to answer significant questions.

Aberbach does make an interesting case that Congress has used its increased resources to engage in more energetic oversight and that they have done so in response to a changed incentive structure. Thus, although he does not fully succeed in separating his results from the findings of those who argue that oversight is done only in response to external agitation, he provides, using complementary data sets, a useful account of the growth of oversight activity between 1961 and 1984 and the variety of forms in which it occurs.

DIANA EVANS

Trinity College

Universities and the Capitalist State: Corporate Liberalism and the Reconstruction of American Higher Education, 1894–1928. By Clyde W. Barrow. Madison: University of Wisconsin Press, 1990. 329p. $39.50 cloth, $17.25 paper.

This volume presents an interpretive history of the development of higher education in the United States during the early decades of this century. The author, basing his analysis on traditional Marxist theory, sees the development of U.S. universities as one aspect of the triumph of corporate capitalism in U.S. politics.

Just as Marx argued that the history of societal development is based on the conflict between labor and ownership, Barrow argues that the development of U.S. colleges and universities is grounded in the battle between corporations and intellectual proletarians. The control of the professorate "consists of a structured institutional capacity to regulate intellectuals through market forces, rather than control them as individuals or as an oppressed group" (p. 252).

Barrow argues that the development of U.S. universities follows the same stages found in other areas of U.S. economic life: "concentration of the means of mental production, centralization and bureaucratization of administrative control, the construction of national academic markets, and the rationalization of market relations between competing institutions" (p. 31).

Within individual institutions, the battle for control pitted the traditonal view of faculty governance (with a faculty often "radical" or critical of capitalist economics) versus the power of boards of trustees (peopled by wealthy capitalists and committed to the status quo of corporate capitalism).

On a broader scale, the ideology of corporate control was diffused throughout the country with the development of national educational foundations supported by the corporate United States. The Carnegie Foundation for the Advancement of Learning and the General Education Board were supported by major financial groups (Barrow highlights three: Morgan, Rockefeller, and Kuhn-Loeb) and disseminated an ideology supportive of corporate capitalism.

This ideology of corporate capitalism had both internal and external dimensions. Internally, the principles of scientific management were applied to the governance and structure of universities, establishing lines of authority and measures for efficiency and productivity. Externally, universities and their faculty members were to stay within the limits of their expertise and not involve themselves in "politics." The concept of academic freedom, which was seen by faculty as protecting the professorate's intellectual integrity, was used by boards of trustees as a tool to control racalcitrant faculty members.

As he does throughout this work, Barrow provides a rich and detailed discussion of vignettes from the historical record, outlining three models of academic freedom: the "Ely model" encouraged radicals to temper their political and economic views and to focus on the personal dimension of their academic careers; the "Beemis confrontation model" provided an example of a well-known economist who became "an academic pariah" after being fired by the University of Chicago; the "Commons model" showed that one could be bought off with the promise of promotion and an upwardly mobile academic career.

Placing what Barrow has done in a broader context, one can say that the modern university is not only subject to the state control of capitalist society but has become part of the apparatus of state control. As Barrow argues early on, *state* is no longer synonymous with *government*. The state is now "a social-industrial complex of overlapping public (and even nominally private) associations that formulate policy, exercise regulatory authority, and assist in social control from a variety of independent institutional centers" (p. 4).

This historical analysis relies on Marxist theory, but it is not a work of theory itself. It offers a case study of the development of U.S. universities using Marxist theory, but it does not provide an empirical validation of that theory or its concepts. It is a thoroughly researched and richly textured account of the historical interconnections between the modern university, government, and the private sector in the United States.

STEVEN D. EALY

Armstrong State College

In the Shadow of the Dome: Chronicles of a Capitol Hill Aide. By Mark Bisnow. New York: William Morrow, 1990. 319p. $20.95.

This is a book of interesting tales about the author's experiences as a Capitol Hill aide during the 1970s and 1980s, and as aide to candidate John B. Anderson in the 1980 presidential campaign. Having served on the staff of Democratic congressmen Don Bonker and Henry Reuss and Republican senators John Heinz and Robert Dole, Bisnow writes with an inside perspective that encompasses both chambers and parties, as well as various levels of member influence. Among the rich details, a "stafflation" theme does emerge: the fourth branch is powerful, if not supreme. While the book lacks profound theoretical insights into Congress, it is a source of useful contrasts that illuminate the internal operation of the two chambers.

The most obvious contrast emerging from Bisnow's account is the difference between the House and Senate staff experiences. Chapter 10, for instance, provides a detailed job description for legislative assistants in the House. In the House, legislative assistants (LAs) are generally required to cover more issue space, answer more constituent mail, and (due to their youth) face more constraints on their advisory role. Still, the interoffice atmosphere in the lower chamber is generally more informal, and staffers communicate with the members more freely. In the Senate, the interoffice atmosphere is more formal, and the expanded duties and larger staffs require and permit greater specialization.

A second insightful comparison is the difference between serving junior and senior members. Through a series of nice examples, Bisnow illustrates the well-known fact that much of a senior member's work involves committees and committee staff. Due to their junior rank, younger members must seek to operate through more indirect channels—soliciting senior members for help (Bonker) or marching directly to the floor (Heinz). Chapter 23 describes Bisnow's work for Henry Reuss on the Joint Economic Committee during the early years of the Reagan administration. Controversial clashes with the administration's supply-siders were a staple of the committee hearings under Reuss's direction. Chapters 25 and 26 describe Bisnow's work for Republican leader Robert Dole highlighting leadership quarrels on the floor of the Senate from the perspective of the staff involved. The discussion of the negotiations behind the trade legislation in 1986 and 1987 provides additional evidence for the supremacy of the fourth branch.

A final important contrast is between serving on personal, and serving on committee, staffs. Bisnow held a variety of jobs on the Hill, as both policy specialist and political generalist. The difference between the two roles can be described as an orientation toward serving the stateside constituency as compared to the committee staffer's role as expert adviser on matters of national importance. But the two are sometimes mixed. His experience on the Anderson campaign was that of a political generalist who was conveniently placed on a committee payroll.

As an insider myself I found *In the Shadow of the Dome* to be an accurate account of how congressional staffers operate in a myriad of situations. In this sense, the book is not a misleading portrayal of staff power, nor is it lacking breadth; but it does lack depth and focus. The chapters are short, and one must look closely to see how the amalgam of stories in the first twenty-eight chapters add up to the conclusions in the last. Bisnow concludes that "the consequence of large staffs is that they empower Congress to take a broader and more detailed role in policymaking, often reflecting the interest of aides as much as that of members" (p. 308). Congress has become an "alternative administration" more than the deliberative body it was intended to be. These are interesting remarks; but earlier, Bisnow seemed less certain about the adverse consequences of stafflation and considers staff to be central to administrative oversight, policy making, and representation.

Finally, Bisnow's experience, broad as it is, remains context-bound in certain instances. At one point, Bisnow discounts the importance of party loyalty among staff. As a registered independent, who often jumped parties and worked for moderate members on both sides, he considers party loyalty to be of secondary importance. However, in reality, party loyalty is very important in the sizable nonmoderate wings of both congressional parties. In these offices, ideology is often the principal litmus test determining the hiring decision. Bisnow acknowledges that staff are self-selected to apply only at certain offices; but it is common for interviewers to ask about a prospect's political background and views, and it is no accident that the application forms distributed by the Senate Republican Policy Committee on behalf of Republican offices actually require applicants to position themselves ideologically on a number of key issues. This selection process is probably no trivial influence on policy outputs and should not be labeled of secondary importance without further empirical work.

In the Shadow of the Dome is a book to be read by outsiders who want to learn about the inner workings of the Congress or are considering a job there. For the uninitiated, Bisnow's revelations about staff power and member activity may even come as shock from time to time. For insiders, Bisnow's account of the Hill contributes to an understanding of the institution by unmasking similarities and differences across committees and members that are not visible from within one's own staff experience.

JIM GIMPEL

Minority Staff, Senate Labor Committee

The President in the Legislative Arena. By Jon R. Bond and Richard Fleisher. Chicago: University of Chicago Press, 1990. 253p. $47.00 cloth, $16.95 paper.

Legislating Together: The White House and Capitol Hill from Eisenhower to Reagan. By Mark A. Peterson. Cambridge: Harvard University Press, 1990. $32.50.

Legislating Together signals a departure from descriptions of the relationship between the president and Congress that emphasize conflict to the exclusion of cooperation. *The President in the Legislative Arena* emphasizes conflict insofar as it is concerned with the factors that condition presidential success in Congress. Both studies increase our understanding of the interactions between the two branches.

Peterson's study goes way beyond description to a specification of the factors that condition congressional reactions to presidential proposals. He codes the eventual outcome of presidential legislative proposals along a dimension of cooperation-conflict between the two institutions. The coding distinguishes whether a given proposal is accepted as submitted, modified in the process of reaching a compromise that is dominated by either presidential or congressional points of view, rejected outright, or ignored entirely by the Congress.

The study uses a random sample of 299 domestic legislative proposals submitted between 1961 and 1988. Peterson's explanation employs measures in the areas of institutional context (e.g., congressional decentralization, aggregate public attachment to political parties, partisan linkages among elected officials, and the elaboration of the interest group system); dynamic political context (e.g., period with the presidential term, popular vote for the president, the effect of presidential coattails in House elections, the president's party's strength in the House and Senate, and public approval of presidential job performance); economic performance indicators (e.g., changes in gross national product, size of the federal deficit, unemployment, and inflation); and characteristic of the legislative proposal itself (e.g., the size of its budgetary impact, whether it is a policy departure, its priority in the president's legislative package, and the volume of legislative related speeches and messages the president has delivered with reference to the proposal). Peterson also employs indicators of public sentiment about the issue to help explain the level of conflict generated when they are dealt with in Congress.

These measures are used in an array of bivariate and multivariate analyses that determine the sources of the treatment received in Congress by presidential proposals. The bivariate analyses set the stage for the multivariate analyses. I have two minor complaints about the bivariate section of the book. The presentation is somewhat inconsistent; not all of the bivariate analyses are presented as tables and there is no apparent rationale for which analyses are offered in tabular form. A bit more vexing is the absence of an analysis of the relationship of the outcome of presidential proposals over time. At several points in the book the idea is mentioned that there may be a trend in the way in which presidential proposals have been dealt with in this period. The point is also discussed in the concluding chapter. But, curiously, the notion of a trend as such is not examined.

The multivariate analyses get to the heart of the matter. Peterson finds that a shift in control of the House from the president's party to the opposition increases conflict but that the level of conflict is unrelated to shifts in the party controlling the Senate. The two chambers show different relationships between the number of seats held by the president's party when it is the majority party and legislative cooperation. In the House an increase in seats for a narrow majority (218–270) *increases* conflict; seats added beyond 270 decreases conflict. In the Senate the curvilinear relationship is reversed. With 51–60 seats held by the president's party, added seats *decrease* conflict; beyond 60 seats, conflict increases. Unexpecedly, the level of public support for the president—measured by the Gallup question on "approval" of the president's handling of the job—is *positively* associated with conflict between the White House and the Congress. Given scholarly attention to this relationship, the lack of discussion of this finding is troubling. Among the economic variables, increases in unemployment, inflation, and the deficit contribute to conflict between the two branches; the level of unemployment tends to mitigate conflict. Policy innovation and novelty tend to increase congressional resistance to the president. Finally, the decentralization of power within Congress (measured as a compound of the number of committees and subcommittees, the size of committee and subcommittee staffs, and a measure of congressional seniority) tends to promote congressional obduracy.

Bond and Fleisher concentrate on party, ideology, and the actions of congressional and committee leadership in their explanation of the level of success or failure of a president's legislative program. Their multivariate analyses indicate that a president does better with the support of both party and committee leaders. Disagreement among congressional leaders hurts the president's program if it takes place within his party and helps his program if it is the opposition that is divided. The study examines the effect of these structural variables on presidential legislative success in both the House and Senate when his party is, or is not, in control of one or both chambers. Majority party status helps but a minority president who gains the support of both party and committee leadership in Congress can achieve a fair measure of legislative success.

These general analyses are augmented with the introduction of variables of policy context and presidency. They investigate whether presidents are more likely to succeed in Congress on foreign policy than domestic policy, whether presidential leadership skills matter, and whether popularity is a presidential asset. Apart from the boost derived from being a highly popular and legislatively skilled president, these factors offer no consistent augmentation of a president's capacity to attain legislative goals.

These two books are important steps on the road to a detailed and systematic understanding of presidential–congressional conflict and cooperation. They overlap to some degree in their models of the political processes linking the two branches, but each study contributes a somewhat different perspective. A logical next step would be a blending of the two perspectives and the attempt to model the sources of congressional leadership support that both books find indispensable to presidential success.

RICHARD A. BRODY

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Regulating the Regulators: An Introduction to the Legislative Oversight of Administrative Rulemaking. By James R. Bowers, New York: Praeger, 1990. 140p. $38.95.

For some 40 years scholars writing about legislative oversight of bureaucracy have predictably begun with a lament about the dearth of scholarship on the subject. These scholars will have to find a new opening line. Legislative oversight of bureaucracy, long neglected by scholars, is now regularly studied at the congressional level. A similar pattern of research is emerging in the states. This book is a useful part of that development.

Bowers introduces his book with some astute comments about legislative oversight and the separation of powers. The major portion of the book is devoted to a case study of legislative review of administrative rule making in Illinois. Inclusive legislative review of administrative rule making is not practiced in Congress but is widely used in the states. Over 80% of the states provide for some form of it. Legislative review is a highly visible form of oversight in the states; it merits more attention than it has received. Bowers's volume is especially welcome for that reason.

Bowers finds that the two primary incentives for legislative oversight of administrative rule making are institutions and policies. Legislatures try to preserve institutional power in an age of increasing delegation to administrators. Legislators try to protect the interests that they feel have been abused by administrative rule making. The former motivation is general, the latter deals with particulars. In oversight efforts (as frequently elsewhere) specific incentives prove to be more potent instrumentally while general incentives retain an important symbolic value.

The major insights in the book will gain nods of recognition from scholars whose work deals with the legislative process. Others may be even more impressed. Bowers finds, first, that in legislatures, as elsewhere, role expectations do not coincide with role behavior. In Bowers's case study many members of the Joint Committee on Administrative Rules (JCAR), the subject of his work, simply do not work hard at carrying out their obligations to oversee unless specific policy considerations intervene. The gap between expectations and behavior for many members of the JCAR is a large one. Much of the literature on oversight in Congress points in the same direction. Second, nature sometimes does abhor a vacuum. On the JCAR, legislative indifference (hence nonperformance) enhances the role of the committee staff. The staff came to play a crucial role in many aspects of committee work. In Congress, staff set the tone less often. Third, legislative effectiveness is related to previous patterns of legislative-administrative interaction. An agency that feels that it is being treated unfairly will try to find ways to resist legislative oversight efforts. The literature on

Congress does indeed posit the importance of context in explaining oversight. Finally, much committee and staff effort is aimed at self-promotion—in this case convincing the whole legislature that executive abuse is a live option and that legislative powers need to be protected. This form of rhetoric is a staple of legislative life both at the national and state level.

If the virtues of this book are clear, so are its limits. One need not linger over the dangers of attempting to generalize from limited case study evidence. Bowers, it should be noted, is very sensitive to this problem. One might wish for more attention to anticipated reactions even if committee review authority is mainly advisory. The actual impact of oversight efforts deserves more attention here, just as it does in the entire body of literature on oversight.

Readers who appreciate a carefully and artfully constructed case study presented in a clear and concise manner will value this book and learn from it. Those whose predilections run toward rational choice approaches in studying oversight will surely be uncomfortable with Bowers's type of analysis, but they can still glean some possibilities for interesting propositions therein. This book is a solid contribution to the research on legislative oversight in the states.

Bowers's research also has some policy implications. For example, those who argue for oversight units separate from the normal committee structure in legislatures should be sobered by the evidence that Bowers provides.

MORRIS S. OGUL

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Marbury v. Madison and Judicial Review. By Robert Lowry Clinton. Lawrence, KS: University of Kansas, 1989. 332p. $35.00.

Unhappy with the activism of the modern Supreme Court yet fearing that either anarchy or executive tyranny might result from the freedom of elected officials to disregard judicial decisions they believed unconstitutional, Robert Clinton proposes a middle course. He would permit the present degrees of judicial discretion and finality only in (1) cases of a judiciary nature (e.g., questions relating to Article III and the rights of criminal defendants) and (2) the few cases of a nonjudiciary nature that involve provisions whose meaning and application are clear (as in John Marshall's contract clause cases, according to Clinton). The Court would defer to Congress and the states in all controversial cases of a nonjudiciary nature (e.g., most matters relating to federalism, speech, and privacy). Clinton calls his proposal the theory of *coordinate review.* He thinks it would give the judiciary final say only over some constitutional questions, not (as does the modern doctrine of judicial review) virtually all constitutional questions. Clinton contends at length that coordinate review is the traditional understanding in the United States. He says it best expresses the principles of authorities like Blackstone, state precedents during the Confederation period, the Federal Convention, *The Federalist,* most nineteenth-century constitutional precedents before the era of substantive due process, and, above all, Marshall's opinion in *Marbury* v. *Madison.*

Clinton argues that historians of the Progressive and later periods have misrepresented *Marbury* as both a case of, and the legitimating precedent for, judicial activism or result-oriented constitutional adjudication heedless of the line between law and politics. He calls this "reinterpretation" of *Marbury* a "myth" and a Platonic "noble lie" grounded in moral relativism and a denial of judicial objectivity. Exposing the myth is a matter of more than historical interest for Clinton. He charges that the mythical *Marbury* has subordinated the interests of democracy to the interests of academic and judicial elites, inhibited broad public discussion of constitutional issues, thrown constitutional doctrines into confusion, and undermined the legitimacy of judicial review itself. But instead of submitting the empirical evidence (say, of comparative doctrinal confusion or poorer levels of public discussion) and the normative analyses (of constitutionalism and democracy, for example) needed to support these controversial charges and motivate reform, he puts his greatest effort into a legal history of *Marbury.* He tries to justify his preference for legal history over constitutional theory by asserting (not arguing) that precedent plays the greater causal role in the development of judicial practice. He thus assumes (still without argument) the practical and conceptual separability of theory and precedent.

He also assumes a readership that shares his unexamined conception of democracy and finds normative significance in historical events, beliefs, and practices.

Clinton has succeeded in writing a book whose literary qualities are an impressive cut above the average for recent writing in constitutional studies. He submits rewarding accounts of the *Marbury* litigation, Justice Gibson's famous opinion in *Eakin* v. *Raub*, and the Progressive era debate on the scope and legitimacy of judicial review. He scores heavily by exposing the unreflective moral relativism of academics who deny the possibility of principled judicial choice—the dogma behind the usual assessment of *Marbury* as an instance of political jurisprudence. Yet the book has serious shortcomings from both historical and theoretical points of view.

First, there is much irony in Clinton's critique of academic moral relativism. Instead of defending the possibility of principled (and therefore "judicial") judgment in controversial cases, Clinton would narrow the scope of judicial review on the assumption that such a narrowing is necessary to avoid policy making (a "legislative" function) by judges (see pp. 70–71, 100–101). This assumption suggests the moral skeptic's contention that policy choices are unavoidable in hard cases. In addition, moral conventionalism, a form of moral relativism, seems implicit in Clinton's preference for constitutional reasoning through historical research and the citation of precedent rather than the activity of fresh moral argumentation (pp. 10, 221–22, 226). Clinton fails to confront arguments that conventionalist conceptions of constitutional meaning (and corresponding restrictions on judicial power) are inconsistent with the moral realism of the founding generation, *The Federalist's* elevation of substantive ends over institutional forms, and Marshall's preference for legal argumentation directed more to the practical commonsense of his readers than to their respect for formal authority.

Marshall is a problem for Clinton. Like other judicial conservatives who are unable to reconcile an across-the-board judicial assertiveness with the classical distinction between judging and policy making, Clinton can enlist Marshall only by reconstructing the record. To support his claim that traditional judges were less activist than modern judges, Clinton tries to reduce Marshall's nationalism in *McCulloch* v. *Maryland* to "judicial deference to an elected national legislature" (p. 195). He also treats Marshall's decisions in *Fletcher* v. *Peck* and *Dartmouth College* v. *Woodward* as non-judiciary questions involving "*explicit* violations of relatively unambiguous constitutional limitations" (p. 118; emphasis mine). Yet by going on to offer a substantive defense of the last two decisions, Clinton concedes what he cannot plausibly deny: that the issues were, at the very least, matters of reasonable controversy—which, by Clinton's criteria, should have condemned Marshall's decisions as activist. As for *McCulloch*, Clinton ignores Marshall's explicit statements regarding the Court's continuing duty to draw the line between state and national powers, a duty that includes declaring that pretextual uses of congressional power are "not the law of the land." Clinton's focus on a holding of McCulloch causes him to ignore what Marshall says and does in the *opinion*. Marshall does more than defer to Congress, he rationalizes Congress's action in terms of a controversial general theory of national authority. The distinction between passive deference and affirmative rationalization is important because a judicial power to approve is, under appropriate circumstances and criteria, a power to disapprove—hence Marshall's assumption that it is possible for Congress to exceed its powers and his assurance that on such occasions the Court will perform its "painful duty."

The best that can be said for Clinton's interpretations of Marshall's decisions and other historical material, from the founding to the present, is that they are just that—interpretations. They compete with other interpretations. Mere assertions regarding the demands of unanalyzed concepts like "democracy" and cases of a "judiciary nature" will not suffice as reasons for preferring Clinton's interpretations. Clinton's assertions presuppose answers to questions like the proper uses of history in constitutional controversy, the nature of constitutional meaning and interpretation, the nature of constitutional democracy and other values, and our knowledge of such values. Contemporary constitutional theory has exposed the urgency and complexity of such questions. For the most part, Clinton's book either ignores or begs these questions.

SOTIRIOS A. BARBER

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Language Planning and Social Change. By Robert L. Cooper. New York: Cambridge, 1990. 224p. $54.50 cloth, $14.95 paper.

The English-Only Question. By Dennis Baron. New Haven: Yale University Press, 1990. 247p. $22.50.

Forked Tongue: The Politics of Bilingual Education. By Rosalie Pedalino Porter. New York: Basic Books, 1990. 298p. $22.95.

"The relationship between language and politics is so obvious that we hardly think about it," wrote Brian Weinstein as the first words of *The Civic Tongue* (1983). As Weinstein went on to show, the relationship between language and politics is not obvious at all. So basic are the unresolved issues that current academic scholarship still debates whether language has any effect on politics, whether politics can have any effect on language, and whether anyone cares enough about language to undertake political action directed at producing linguistic consequences.

One of the main subjects connecting language and politics is language policy. And Robert L. Cooper's *Language Planning and Social Change* is the most carefully crafted survey to date of this field, which he, like many others, calls "language planning." A survey of a mature field would describe several of its influential theoretical models and many of its major empirically supported generalizations; but, as Cooper admits, this is not a mature field, so the book focuses instead on describing illustrative cases of language planning, defining the field's boundaries and subfields, arguing for criteria that research in the field should satisfy, and describing areas of theory outside the field that might be useful inspirations for it. The cases Cooper describes at greatest length are the founding of the French Academy, the promotion of Hebrew in Palestine, the campaign against sexist usage in English, and the perfunctory adoption of native languages as vehicles of literacy teaching in revolutionary Ethiopia. Cooper's definition of language planning is "deliberate efforts to influence the behavior of others with respect to the acquisition, structure, or functional allocation of their language codes." The subfields into which he partitions the field are status planning (such as designating official languages), corpus planning (such as designing a writing system for a language), and acquisition planning (such as offering a foreign language in a school curriculum). The criteria of research quality he advocates are adequacy of description, prediction, explanation, and theory. The other areas of theory he recommends attending to are the diffusion of innovation, marketing, power, decision making, and social change. Does this work on case description, definition, classification, and theoretical referral add to our knowledge or help those who seek to produce new knowledge? I have my doubts. Cooper (who has published significant work on how languages spread and contract) could do more good if he expended the same amount of effort executing the kind of research that he advocates.

Given that "language planning" has been a self-conscious field since the 1960s, it has generated some claims of general truth; and Cooper sprinkles several of these claims through his book, supported by illustrative cases. One iron law is that "once a language has passed out of all use whatsoever, it remains dead" (p. 12). Another is that "language planning . . . is typically, perhaps always, directed ultimately towards nonlinguistic ends" (p. 35). It is also "a messy affair—ad hoc, haphazard, and emotionally driven" rather than "systematic, rational, theory-driven" (p. 41). Language planners rely "on intuition"; they make "little if any effort to determine the motivation of the different target groups to accept the planned innovation" (p. 75). Such motivations, however, are crucial, because "few language-planning decisions can be implemented by fiat" (p. 75). Even if poorly executed, language planning is "employed to maintain or strengthen elite power . . . or the power of counterelites" (pp. 81–82). But inertia is a constraint: "A government continues to use the working languages which have always been used, unless doing so threatens its legitimacy, encourages a counterelite, or otherwise undermines its ability to rule" (p. 103). Standards of appropriate language usage are more often accepted than obeyed (pp. 134–35). Prescriptive grammarians (such as Samuel Johnson) help to diffuse existing standards but don't succeed in revising standards (p. 146). Nonstandard language forms, even when publicly disparaged, enjoy "covert prestige" among their users (p. 179). Terminologists seek to promote the incompatible goals of mass accessibility, which requires coining, and international communication, which requires borrowing (p. 151). Occasionally, Cooper also empirically refutes laws asserted by others, such as Spenser's dictum that "it hathe bene ever the use of the Conquerour to despise the Language of the Conquered and to force him by all means to learne his"—falsified by the Italians in Ethiopia, the Ottomans in the Middle East, and the Manchus in China (pp. 110–11).

To appreciate the "emotionally driven" character of language policy, one needs only to witness today's debates in the United States about whether to declare English the official language and whether to require public schools to provide bilingual education. The debaters, typically, are stubbornly committed; but some hide their biases behind uncommitted-sounding titles, such as *Forked Tongue: The Politics of Bilingual Education* and *The English-Only Question.*

The biases of these two debates are generally opposite. As a first approximation, Porter has an assimilationist bias, and Baron has a pluralist bias. Porter argues for the immersion of language-minority children in school programs where the society's dominant language is the medium of instruction. Baron argues that racism and nativism have motivated a long history of minority-language suppression in the United States, and he advocates the recognition and use of minority languages by governments in schools and other institutions. Both authors rely on largely anecdotal evidence. Porter's bias is not as simple as Baron's, since Porter, too, opposes (though weakly) the declaration of English as the official language of the United States and actually advocates bilingual education if it treats two language groups equally, teaching each the other's language. Thus, the real incompatibility between these two advocates relates to whether it is feasible and beneficial to immerse all kinds of monolingual children (not only middle-class children of well-educated parents) into non-native-medium school programs. Porter and Baron are confident their opposite answers are correct, but Cooper's answer is that we don't yet know. He describes various obstacles to definitive experimental studies. He also notes that what is called "bilingual education" in the United States actually spends little time (in one study it was measured as 8% of the total time) using the pupil's first language (pp. 52–53), leading to the suggestion that bilingual education might work if it were ever tried.

JONATHAN POOL

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American Federalism: Competition among Governments. By Thomas R. Dye. Lexington, MA: D.C. Heath, 1990. 219p. $30.00 cloth, $16.95 paper.

Thomas R. Dye's new study of American federalism begins with ambitious goals. "The ideas set forth in this book," he writes in his preface, "challenge fifty years of scholarship on American federalism. The academic euphemisms used to legitimize centralized government . . . are explicitly rejected in favor of the founders' original notion of encouraging opposite and rival interests within the structure of government. Many in the academic establishment will feel threatened" (p. xvi). Later, Dye expresses gratitude to his publisher for publishing "a book that challenges the accepted academic orthodoxy." Alas, the chapters that follow these large promises do little to present ideas that are either challenging or threatening to an "establishment" that surely must include the author himself.

The book is composed of two different parts. The first is an argument for governance through multiple agencies rather than a single agency, built upon ideas borrowed from public choice theorists. In the first and last chapters, Dye rejects models of "dual," "cooperative," "fiscal," and "centralized" federalism in favor of *competitive federalism,* which "envisions a marketplace for governments where consumer–taxpayers can voluntarily choose the public goods and service[s] they prefer, at the cost they wish to pay, by locating in the governmental jurisdiction that best fits their policy preferences. In this model of federalism, state and local governments compete for consumer-taxpayers by offering the best array of public goods and services at the lowest possible costs" (p. 14). This is the model that Dye believes to be challenging and threatening, but it is not at all clear why a model formalized as long ago as 1956 by Charles Tiebout and widely debated since that time should now be regarded as either new or threatening. Dye himself recognizes that "competitive federalism is more a theoretical model than a description of the American federal system" (p. 189), but he defends the model as a representation of desirable values: efficiency in the production of public services, responsiveness to citizen demands for services, and, above all, a defense against the tyranny that is possible in all governmental systems.

Students of American federalism will find these arguments familiar, but Dye does little to enhance their appeal. Thus, while he argues that multiple governments with varied mixes of services "provide comparative information to both officials and taxpayers about government services and their costs" (pp. 16, 96, 178), he offers no evidence whatsoever for this assertion. On the contrary, the evidence he does cite asserts "the monumental ignorance of the U.S. public concerning not only the basic structure and economics of taxes, but the amounts they themselves pay and the government services that they receive" (p. 69). Nor does the author effectively address the weakness of the mobility assumption built into the competitive federalism model. He offers a table showing that nearly 40% of all U.S. citizens moved in the 1980–85 period but fewer than 18% to a different county and fewer than 9% to a different state (p. 179). It may well be, as he asserts, that "the United States is the most mobile society in the world" (p. 178), but it seems doubtful that these rates are robust enough to sustain the theoretical weight Dye assigns to them.

Sandwiched between the first and last chapters are six chapters that constitute the second part of the book, in which Dye uses cross-sectional and time series regression analyses to examine the "determinants" of various state tax and expenditure policies, including a chapter analyzing the effect of these policies on economic growth. Those familiar with Dye's earlier contributions to this style of analysis will find familiar themes here as well: wealth is the most important determinant of state policies; political parties do not matter much; and taxes appear to restrict growth, but it is difficult to identify state policies that encourage growth. Since these analyses were based on a comprehensive data set covering all states from 1957 to 1985, it is perhaps interesting to note Dye's conclusion that "there is no convincing evidence that the policy preferences of the states are becoming homogenized or even that federal intervention is forcing uniform policies on them" (p. 41). In short, state policy differences continue to exist and continue to offer interesting opportunities for analysis.

Apart from the general familiarity of these arguments, the major shortcoming of the book is that its two analytic parts do not articulate well with one another. Dye's hostility to centralized government shows through clearly enough in chapter 4, "Federal Interventions," replete with loaded language such as "baneful effects" (p. 107) and "blackmail" (p. 108). But apart from such expressions of normative preferences, the competitive federalism model does not focus the analyses in any obvious way, nor do the correlation and regression exercises address the central theoretical issues posed by the model. The result is a book whose separate parts remain largely unintegrated. This is not to say that the separate parts are without value: market proponents will appreciate the normative argument and data analysts will find items of interest in the middle chapters. Bringing the theory together with relevant data, on the other hand, remains a task for the future.

THOMAS J. ANTON

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The Rise of the Entrepreneurial State: State and Local Economic Development Policies in the United States. By Peter K. Eisinger. Madison: University of Wisconsin Press, 1988. 336p. $40.00 cloth, $17.50 paper.

The Politics of Plant Closings. By John Portz. Lawrence: University Press of Kansas, 1990. 214p. $29.95 cloth, $12.95 paper.

While subnational governments have long been involved in fashioning policies to attract businesses, the intensity of the competition between them, the range of their policies, and the stakes involved increased during the last two decades. In turn, a large number of social scientists turned attention to the field of economic development; and during the 1980s economic development became one of the most important areas of research in the study of state and local politics.

Eisinger's book is perhaps the most complete scholarly description of state economic development activities yet published. It is encyclopedic in the range of topics it covers and in the policies it surveys. Eisinger is mostly concerned with economic development policies at the state level, with some reference to economic development policies at the municipal level.

Eisinger argues that during the 1980s there was a profound shift in the mix of policies and expectations in the area of economic policy. His main contribution hinges on the distinction between supply-side inducements (usually aimed at existing firms) versus demand-side policies designed to accommodate, anticipate, or even create demand for new products. Briefly, supply-side policies are based on theories of locational advantage—firms locate in places where input factor costs are lowest. States and cities have traditionally employed policies seeking to reduce the costs of the factors over which they have the greatest control, usually taxes (e.g., tax abatements) and land (e.g., industrial-commercial site development). Supply-side policies have been frequently criticized as ineffective and subject to a form of the "winner's curse"—hypercompetition between subnational governments can lead them to grant subsidies in excess of returns.

According to Eisinger, as criticisms mounted and as the United States moved into a more competitive world market, state governments became more entrepreneurial—they began to develop demand-side policies. As entrepreneurs, subnational governments actively *discover* and *create* investment opportunities and assess and compare alternative uses of their limited capital. Demand-side policies include policies such as state-run venture capital funds and state efforts to develop export markets for products produced by local firms. The distinguishing characteristic of these policies—what makes them "entrepreneurial"—is their concern for creating *new* opportunities and *new* firms. These policies also provide a more equal partnership between the private and public sectors, redefining the traditional primacy of private capital vis-à-vis government.

Eisinger has provided a valuable service in collating a wide range of information about the evolution of the repertoire state governments bring to bear in this policy domain. The distinction between supply-side and demand-side policies is important as an organizing heuristic (although the choice of terms seems outdated, given recent reevaluations of the Reaganomics that popularized them). But Eisinger's book falls short at certain key points. For example, while the concept of the "entrepreneurial state" is unquestionably an important insight, Eisinger does not develop an extensive theoretical framework for understanding the politics of entrepreneurial economic development policy. Second, ultimately, *states* are not entrepreneurs, *individuals* are. If states have become entrepreneurial, it is because state officials have become entrepreneurs. There is no discussion of public sector entrepreneurs—individuals who have pursued the newer, more aggressive policies that Eisinger documents. Thus, while Eisinger shows that entrepreneurial policies are increasingly common, he has not analyzed the political process by which entrepreneurship has come to be: How have entrepreneurial policies diffused? What are the conditions under which states have adopted such policies? How does the entrepreneurial state nurture entrepreneurial politicians and bureaucrats? These types of questions must be addressed in order to flesh out Eisinger's insights.

Like Eisinger, Portz is also concerned with the range of possible activities that subnational governments can undertake in the area of economic development. While Eisinger concentrated on state governments, Portz is more interested in local governments. Specifically, Portz is concerned with the political dynamics of communities facing economic distress brought about by the closing of a major industrial facility. Portz argues that local governments can choose one of three basic roles: *bystander* (essentially a passive minimal role), *offsetter* (in which local government provides key economic resources, such as financial assistance packages to business firms or retraining to displaced workers), and *player* (in which the local government asserts an active role in the decision process).

Portz roots his analysis in a class-based "political economy" framework and tries to organize his argument across three levels of analysis: economic accumulation (in which the primacy of capital is a given), the institutional system (which provides the arenas for conflict), and political alliance formation and mobilization (including entrepreneurial activity, coalition building, and social movements).

While the bulk of Portz's book consists of case studies of plant closing in three areas (Louisville, Waterloo, and Mon Valley) buttressed by mini-case studies, the work must ultimately be judged by the usefulness of its theory. Portz recognizes that his basic theoretical framework resembles Clarence Stone's concept of "regime politics." Further, Portz frequently waffles in the application of his own framework. As a result, I did not come away with a set of guidelines or concepts by which to untangle the complexity of the local government–local political economy nexus. His typology (bystander, offset, player) should play such a role; but Portz notes that "each policy type is more complex than an individual case study would indicate—rarely are two plant closing experiences that fit under one policy type exactly alike and it is often the case that actual plant closing experiences are a composite of two or three types rather than a 'pure' case of only one type. . . . Reality rarely conforms to descriptive typologies" (p. 144).

Within his typology, government as player resembles Eisinger's concept of the entrepreneurial state. But in describing this role, Portz again equivocates: "By taking a player role, government policy makers often become aligned with different groups or organizations in the community. Alignments on one side of the political spectrum yield populist responses, as in the Mon Valley case study, in which government actions favor labor and community groups and oppose the actions of business groups. Alignments on the other side result in corporate responses in which government actions are in cooperation with, or support of, the business corporation and generally lack labor and community involvement. Equally important is the middle ground of government action supported by a combination of business, labor, and community groups. . . . While some policy responses fit in a relatively clear manner, others are not so easily labeled" (p. 169). Certainly, the world is complex, but the role of theory is to cut through complexity and help us order diverse facts and events into a coherent picture. Portz, while presenting some interesting ideas and valuable case studies, has not sustained a rigorous theoretical framework that can be used by other researchers.

Despite their differences, these two books do mirror the state of the art regarding the analysis of economic development policy: case studies dominate and theoretical development is still needed. Eisinger's focus on entrepreneurship may point to one avenue through which such theoretical development may occur.

MARK SCHNEIDER

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Constitutional Diplomacy. By Michael J. Glennon. Princeton: Princeton University Press, 1990. 353p. $35.00.

According to Michael J. Glennon, Ronald Reagan's naval support for Kuwait during the Iran-Iraq war flouted the 1973 War Powers Resolution and exceeded the president's constitutional power. Shortly after the publication of *Constitutional Diplomacy* George Bush responded to the conquest of Kuwait by ordering a hundred thousand armed forces to defend Saudi Arabia, calling up the reserves and directing the U.S. navy to blockade Iraq. Most commentators immediately praised the president's regard for international law enacted by the UN Security Council without recognizing the constitutional problems arising from presidential military action taken without formal congressional approval.

Michael J. Glennon was legal counsel to the Senate Foreign Relations Committee from 1977 to 1980. A foreword by that committee's former chairman J. William Fulbright calls on the Bush administration to respect the Constitution's democratic procedures in making foreign policy. Glennon laments that both Congress and the courts have neglected their constitutional responsibility by allowing the executive to seize the policy making power to initiate war. His constitutional interpretation resembles strict constructionism, relying on the favorable text and original intent of the framers, while challenging customs and Supreme Court precedents that have adapted our fundamental law.

The plain meaning of Articles I and II give Congress far more authority than the president on questions of war and peace. The Constitution may indeed have made the executive commander in chief, but it empowers Congress to raise and support the army and navy, to provide for the common defense, to suppress insurrection and repel invasions, and to declare war. The founders who rebelled against George III's abuse of power intended to establish legislative checks on the executive. Unlike C. Herman Pritchett, Glennon does not regard John Marshall's statement that the president is the nation's "sole organ" of foreign policy as a correct reading of the framers' intent. In context, Glennon finds that Marshall merely considered the president the governement's sole representative able to send and to receive international communications, not that the executive was the sole foreign policy decision maker. In *Little* v. *Barreme* Marshall invalidated a naval seizure of foreign vessels because it exceeded war powers conferred by Congress during the Adams administration.

That 1804 precedent supports Glennon's claim that a twentieth-century Congress when authorizing military action must impose limits on a disproportionately severe response. Glennon also challenges Supreme Court precedents sustaining inherent executive power. Lincoln's civil war blockade of southern ports upheld in *The Prize Cases* did not set precedent for a unilateral executive blockade of a foreign power, such as Iraq.

Justice George Sutherland's *Curtiss-Wright* opinion celebrating inherent presidential power also fails to constitutionalize subsequent executive war-making initiatives. In *Curtiss-Wright* Congress had expressly authorized the action challenged as excessive. Do extraconstitutional inherent sovereign powers in foreign affairs vest in the president or in Congress? Like Louis Fisher and others, Glennon relies heavily on Justice Robert Jackson's concurring opinion in the steel seizure case. When Congress delegates the nation's inherent sovereign power, the President exercises the greatest authority. In the absence of legislative guidance, the President may act within reason. When Congress has expressly regulated the scope of Presidential authority, however, claims of executive prerogative are suspect.

The 1973 War Powers Resolution purports to fulfill congressional responsibility by requiring congressional approval of military intervention abroad. Glennon concludes that poor drafting vitiated the presidential reporting requirement that incumbents have openly disregarded as unconstitutional. The Supreme Court invalidated the act's legislative veto provision. Glennon proposes amendments that would democratize U.S. decisions to use force abroad and expressly confer jurisdiction on federal courts to resolve constitutional disputes over the check on presidential power.

Glennon demonstrates compelling knowledge of historical source material, government documents, and case law. His analysis is so well documented with ample reference to opposing authority, that the work will be a valuable reference even for those who disagree with its conclusions. It is well written throughout and contains flashes of quotable style.

Glennon's forthright and principled defense of constitutionalism will undoubtedly be criticized as outmoded and idealistic, particularly by proponents of stronger executive authority. Certain policy recommendations, while timely, do appear unrealistic in view of the prevailing support for a strong commander in chief. Glennon cannot escape the dilemma of acknowledging past congressional failure while advocating a resurrection of legislative power. Nonetheless, his appeal to democratic values and judicial enforcement of constitutional principles is compelling. Like Louis Henkin, Glennon has authored a work that warrants reading by political science students of the U.S. Constitution and its separated institutions sharing power.

HOWARD TOLLEY, JR.

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A Citizen's Right To Know: Risk Communication and Public Policy. By Susan G. Hadden. Boulder: Westview, 1989. 239p. $27.97.

Susan G. Hadden advocates more thorough *right-to-know* regimes than those that were created in the wake of the Bhopal incident of December 1984. Additionally, and perhaps more notably, she provides an early study of the implementation of right-to-know within the constraints of its formulation and of informational and subnational bureaucratic realities. Yet while the book thoroughly explores the question of what is necessary for right-to-know to be a success, it provides only unconvincing evidence regarding what is sufficient.

The author's edited volume, *Risk Analysis, Institutions, and Public Policy* (1984) has had a founding part within a diverse literature on risk analysis. In her more recent work, she demonstrates a strong familiarity with other scholars, such as Sheila Jasanoff.

Apart from its identity with a phrase familiar to First Amendment scholars, right-to-know has a more recent genesis and a shorter library shelf than *risk*. Yet risk (or, more properly, uncertainty) severely limits what the public *could* know under the best of right-to-know circumstances. For example, in many official-seeming reports the "ranges" of cancer deaths attributed to a given chemical or process reflect only an estimate due to the uncertainty involved in extrapolating from studies in which animals are administered massive dosages. This still-unknown thing is assigned a "range" to make the results seem more credible. Hadden is more eager to explore these data than those who wish only to discuss cultural and psychological risk ironies because, she asserts, a costly right-to-know approach should be continued, amid uncertainty, while current data management practices should be overhauled.

Hadden's repeated invocation of the choosing "citizen" mars the credibility of her approach. With respect to complex information and uncertainty, many "citizens" are unlikely to have the economic or other means to make relocation decisions based on toxic waste or other delayed-risk information. It is more likely that this information will either be ignored or simply cause residents to live in more fear than before.

To Hadden's credit, she readily acknowledges the argument that the usability of great quantities of technical data may be so low that a right-to-know regime may simply overwhelm most citizens. She also discusses, early on, the great volume, unknown time horizons, and infinite interactivity of the chemicals involved. But her concluding discussion seems to advocate sincerely what Murray Edelman and others have seen more analytically, namely, that government and those useful to it exist in large measure to "answer questions" and reassure, irrespective of the truth of the specific tale or ritual doing the reassuring. Certainly her discussion of "informed consent," while qualified in several particulars, remains insulting to those who emerge as subjects, rather than as citizens, under so many sets of circumstances related to this topic.

At various byways in the narrative, Hadden invokes—without making a necessary argument for their modality—situations in which hazardous sources are known and provided only by a right-to-know regime and in which relatively simple citizen-initiated actions remove any risk for them. She has the beginnings of a more empirically supportable argument in her early assertion that right-to-know "is less important for the information that citizens may obtain than for its effects in actually reducing the risks to which citizens are exposed" (p. 15). Yet perhaps the better "deterrence" argument is one that *specifies* this process, indicating, for example, that an information regime, given its activation of plant managers' fears and hunches, would be sufficient to reduce ill-health effects in advance of new scientific certainties, without necessarily stemming other sorts of production. Perhaps the provision and consideration of worst-case scenarios (a common way of handling uncertainty) should also be required, in delayed-risk as well as acute situations (see p. 41). Finally, Hadden has the beginnings of a user-friendly and not overbearing innovation in her suggestion that much public information should be in the form of multifaceted "fact sheets" in question-and-answer format (see p. 196 and accompanying discussion).

Certainly, Hadden's book is important in its detailing of the horribles (and occasional bright spots) attending the passage and implementation of federal (actually intergovernmental) right-to-know. As in other would-have-been-innovative policy areas, the effects of an ever-exploding complexity have both stressed and elaborated U.S. procedural institutions like separation of powers, bicameralism, federalism, and (more recently) privatization—at the expense of both coherent policy and the policy actors' influence (see Senator Durenberger's comment quoted on p. 201n). Perhaps the time has come for policy and process scholars alike to move away from islands of expertise and laundry lists of turf-building incremental policy improvements and toward a process-policy synthesis that rescues the system's performance.

Even following the prescriptive portion of Hadden's analysis, someone considering community right-to-know must wonder whether there are policy goals that should be both doable and done well or else abandoned as false and crippling hopes. A system that is unable to steer its citizens and subjects toward either of these shores is in need of change.

R. CHRISTOPHER PERRY

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To the Right: The Transformation of American Conservatism. By Jerome L. Himmelstein. Berkeley: University of California Press, 1990. 301p. $25.00.

Jerome L. Himmelstein's *To the Right* is a critical examination of the recent successes of the U.S. conservative movement. This study generally does not seek to build up or destroy that movement. In a nonjudgmental fashion the sociologist Himmelstein explains why the political Right achieved prominence in the United States in the late 1970s and 1980s after being largely dismissed for years as a fringe movement that opposed modernity and lacked a positive philosophy.

Himmelstein rejects the view of a U.S. conservatism with an intellectual tradition that draws inspiration from Edmund Burke (pp. 28, 93–94). U.S. conservatism is traced only to the 1950s, particularly to the opponents of the New Deal philosophy and the followers of Senator Joseph McCarthy. Political schisms characterized the political Right and reduced the movement to an opposition force. The "transformation" of U.S. conservatism began through a synthesis of different right-leaning groups: militant and anticommunists, traditionalists, and libertarian defenders of "pristine capitalism" (pp. 29–62). Three developments in the 1970s and 1980s led to the success of the political Right. First, evangelical Christians became politically active and rallied to support conservative figures for public office. Second, corporate conservatives mobilized to fund right-leaning scholars and think tanks. Third, the Republican party experienced a revitalization due in part to the first two developments and to public dismay with Jimmy Carter's leadership (pp. 97–197).

Despite conservatism's "transformation" Himmelstein assesses that very little changed in the leadership and ideology of the Right. The same people, principles, and strategies defined the movement from its 1950s origins to the 1980s triumphs. Accordingly, there is not much "new" about the New Right. Changes in the social context, not changes in conservatism, created the conditions for the political Right's success (pp. 64–65).

Himmelstein concludes that "the New Right's day has passed" (p. 202); that is, the movement failed to have a lasting impact on the nation's policy agenda. On the surface, much of the evidence is persuasive: Ronald Reagan never effectively translated his popular appeal into widespread support for a conservative agenda; the conservative movement ended up in disarray at the conclusion of Reagan's tenure; the much-touted Republican realignment never occurred (pp. 202–10); the Republicans only achieved "selective realignment," while economic voting and dealignment also influenced Reagan-era elections (p. 181); and they succeeded at making "liberal" an unappealing label but failed to turn the country against a Democratic party agenda (p. 209). George Bush's election to the presidency in a year in which conservatives could not unite behind any candidate further demonstrated the failure of the Right to hold power (p. 200).

There is much to commend to political scientists in this volume. The analysis of the realignment thesis and of why conservatives could not unite beyond the Reagan years is compelling. The review of how diverging schools of U.S. conservatism came together for a short time and eventually splintered again makes clear why many conservatives became pessimistic about the movement's prospects.

This study, however, does not provide a complete picture of modern U.S. conservatism. Remarkably, there is either scant attention given to, or no mention at all of, such important "neoconservatives" as Norman Podhoretz, Daniel P. Moynihan, Midge Dector, and Jeanne Kirkpatrick or to the role of *Commentary* magazine. The role that *Commentary*'s writers and such pundits as George F. Will played in popularizing conservative ideas espoused by Ronald Reagan and dismissed by many intellectuals as ridiculous only a few years before the 1980 election is ignored. Conservatives did not fortuitously come to power because of a changed social context. Many conservatives played an important role in creating a context for acceptance, albeit temporarily, of some conservative ideas that had not been taken seriously until the late 1970s. Himmelstein perhaps gives too much credence to New Right leaders at the expense of leading neoconservatives and traditional conservatives: among "conservative" activists cited in the index, those with the largest number of citations (in order) are Jerry Falwell, Pat Robertson, Paul Weyrich, William F. Buckley Jr., Richard Viguerie, and Howard Phillips. There is a lengthy analysis of North Carolina's 1984 senatorial campaign in which incumbent Republican Jesse Helms defeated Democrat James Hunt (pp. 181–97). An interesting and worthwhile analysis in its own right, this section is more of a detour from the general thrust of the book and adds little substantively to the author's broader assessments of U.S. conservatism. The long-term significance of the 1980s conservative triumphs may be understated by Himmelstein. The political dialogue shifted somewhat to the Right on budgetary, defense, and social issues; and this shift will profoundly influence policy decisions for a long time. The Supreme Court likely will be aligned with a conservative constitutional agenda for years. Conservatives couldn't agree on a presidential successor to Reagan; but they still landed the vice presidency, chief of staff, drug czar, and other critical appointments. And conservatives can do what Himmelstein acknowledges they do better than governing: opposing (p. 202). Conservatives may not reign in the post-Reagan years, but their movement is still very potent.

MARK J. ROZELL

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U.S. Senate Decision-Making: The Trade Agreements Act of 1979. By Robert W. Jerome. New York: Greenwood, 1990. 184p. $39.95.

The development of trade agreements is a complex and difficult task among nations. To complicate matters for the United States, the implementation of these agreements is dependent on specific legislation that provides for changes in domestic law necessary to fulfill the international objectives of the trade agreement. All of this takes place under specific rules and time constraints that differ from customary U.S. law-making procedures. Robert W. Jerome, in *U.S. Senate Decision-Making: The Trade Agreements Act of 1979,* takes a look inside this legislative labyrinth and proceeds to examine the theoretical implications for his observations. He concludes by suggesting a plausible hybrid model of Senate decision making for the case of implementation bills that deserves further testing.

When the Tokyo round of the multilateral trade negotiations ended in April of 1979, it signaled the beginning of an accelerated grind of legislative activity that ultimately provided the implementation legislation for the trade agreements. Having served as a legislative director for a member of the Senate Finance Committee and later as an assistant to the undersecretary for international trade in the Commerce Department, Jerome witnessed these activities from the inside.

His book is divided into two sections. The first is a legislative history of the Trade Agreements Act of 1979. This history, though useful, could have benefited from a more elaborate look into the Trade Act of 1974, which set up the rules that governed consideration of the 1979 act. Jerome does provide however, a unique look into the hazy world of Senate background briefings while observing some of the key senators in action. He also sheds light on such arcane fast-track procedures as the "non-mark-up" and the "nonconference." A few simple tables such as a roster of the Senate Finance Committee, a list of the trade acronyms, and a legislative time line would make the narrative clearer to those not familiar with trade matters or the ways of Capitol Hill.

Yet this book is not meant to be another insider's journal. In the second and concluding part of the book, Jerome reviews political decision-making theory and constructs a new model. The author identifies a threefold typology of decision-making theory: *structural* theories such as Graham Allison's rational actor approach try to look to where the decisions are made; *factorial* theories, often used in legislative research, attempt to identify what combination elements are most responsible for an action (e.g., executive agencies, committees, or leaders); and *behavioral* theories attempt to show how decisions are reached—as through the use of Lewis Froman's "serial bargaining" or David Matthews' "folkways."

By developing this typology into a three-stranded "decision rope" model, Jerome is able to call upon a combination of major theories for application in this case study. The sometimes neglected use of time as a theoretical dimension—used here as a time slice in the rope—is then added when explaining specific actions—as when the Finance Committee held public hearings (p. 128). At one time, a structural perspective may be the more accurate in accounting for an action; at other times it may be a combination of theories. The time slice of the decision rope offers an interesting conceptual tool for analyzing the complex world of senate decision making.

Seeking to combine or unify several theories to analyze policy decisions is reminiscent of economist James D. Shaffer's "institutions, behavior, performance" model used in examining food and agricultural policies. A complementary theoretical view as applied to the areas of economic, tax, and energy policy can be found in Douglas Arnold's new book, *The Logic of Congressional Action* (1990).

As the Uruguay round of the multilateral trade negotiations winds down, several of the same policy difficulties that Jerome describes in his book endure. Fast-track procedures will again prescribe congressional actions; moreover, these procedures are now becoming a more common legislative tool. Legislation as diverse as budget reconciliation, military base closings, and humanitarian aid have used fast-track rules.

U.S. Senate Decision-Making focuses needed attention on the evolving role of Congress in trade policy. The author also offers an interesting theoretical model deserving of more testing in the lanes of other fast-track policy regimes. In addition, this book, along with Barbara Sinclair's recent work, *The Transformation of the U.S. Senate* (1989), and John Hibbing and John Peters's new edited collection of essays, *The Changing World of the U.S. Senate* (1990), underscore a refreshing trend toward reexamining the fundamental role of the U.S. Senate in developing and implementing public policies.

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Earl K. Long: The Saga of Uncle Earl and Louisiana Politics. By Michael L. Kurtz and Morgan D. Peoples. Baton Rouge: Louisiana State University Press, 1990. 312p. $24.95.

The Disappearing South? Studies in Regional Change and Continuity. Edited by Robert P. Steed, Laurence W. Moreland, and Tod A. Baker. Tuscaloosa: University of Alabama Press, 1990. 224p. $29.95.

While both of these books deal with southern politics, they contain information and insights of value to political scholars generally. Kurtz and Peoples have provided not only a much-needed antidote to the recent, highly distorted Hollywood treatment of Earl Long but a provocative portrait of a politician whose personal failings were of such legendary proportions as to obscure his equally extraordinary accomplishments. Steed, Moreland, and Baker continue to provide a valuable service by collecting, compiling and providing useful frameworks for some of the excellent work being done on the rapidly changing South.

After decades of research, including hundreds of personal interviews, Kurtz and Peoples have written the most comprehensive portrait the literature offers of Earl Long, a richly gifted but deeply flawed politician. Never questioning the amoral conventions of the political world that he adored, Long had genuine populist sympathies but only one operational ethic: his own political advantage. The results could be beneficent (e.g., enfranchisement of blacks, whom Long early identified as an essential component of his populist coalition) or detrimental (official protection for mobsters and widespread graft). Everything was "right" or "wrong" judged exclusively by the exigencies of the coming contest. Long's saving grace was his lack of hypocrisy. As the authors note, "Earl never made any pretense of supporting the principles and practices of good government" (p. 134). Nor, in dramatic contrast to today's timid politicians, did Long ever attempt to obscure or excuse his bizarre personal life. Indeed, it was his rather novel political theory that at least in Louisiana, voters would quickly tire of bland, good government and happily cycle back into more entertaining, albeit more costly, "bad" government.

The authors emphasize several previously neglected aspects of Long's idiosyncratic career. One is that his programmatic accomplishments as governor (charity hospitals, old age pensions, teacher tenure) far eclipse those of his big brother Huey and, indeed, that many of Huey's legislative successes were due to Earl's superb skills in lobbying the legislature. They also, through research into recently declassified FBI files, document Long's close ties to some of the era's most notorious mob leaders. Of particular interest to political scientists is a convincing challenge to V. O. Key's characterization of Louisiana as a strongly bifactionalized state. Rather, Kurtz and Peoples demonstrate, "political reality has consistently triumphed over factional loyalty" (p. 144), that is, the only thing that ever really mattered in these kaleidoscopic contests was politicians' perception of their own self-interest.

The book is not flawless. There are inaccuracies (it is Orval—not Orville—Faubus; and "joint stump meetings" were certainly not unique to Louisiana) and unsubstantiated statements (that the Long's had the richest political machine in the country or that Louisiana elections were the most expensive or the most corrupt) without any reference to comparative data from other states. The annotation style (one note per paragraph) makes it impossible to detect the precise source of some especially controversial observations; and, at times, the notes and the assertions just don't fit, as when a reference is made to "the most comprehensive study of black voting rights in the postwar South" (p. 198) and the work referenced is *Report of the Secretary of State.* Assertions about Long's accomplishments are unnecessarily repeated (sometimes in identical language) from one chapter to the next, while other material of enormous potential interest (e.g., J. Edgar Hoover's "close friendship" [p. 138] with Long's backer and bagman, businessman Seymour Weiss) is insufficiently examined. Still, this is a fascinating, analytical portrait of a man—and a political system—gone awry.

The Disappearing South? takes a broader and deeper look at contemporary southern politics, and (as one has come to expect from this series) its contribution is substantial. The thematic incoherence that often plagues collections is deftly avoided here with a useful and illuminating format. After a wonderfully wry foreword by John Shelton Reed, the first set of articles presents evidence that the South, politically at least, has been rather thoroughly "nationalized"; the second set of articles argues to the contrary.

Those pointing to the decline of southern distinctiveness base their arguments on evidence gathered from changing patterns of committee assignments in the U.S. Senate and on ideological changes in the southern electorate and among party activists and campaign contributors. There is impressive proof here of the rapid decline of the one-party South and the strong emergence of national partisan models.

The arguments for continued southern distinctiveness are somewhat more specialized, finding the strongest signs of differentiation in beliefs regarding religion, labor unions, and race. As the editors aptly note, the "most noticeable nationalization of southern politics has occurred, and continues to occur, in the region's political institutions and structures," while "the most striking remnants of southern distinctiveness can still be found in patterns of attitudes and behaviors that reflect a broader sociocultural/historical heritage" (pp. 174–75). Interestingly, essays in both sections find evidence that the Republican party, rather than the once-dominant Democrats, "has developed into the vehicle for the perpetuation of southern culture over the past three decades" (p. 133). The high quality and readability of the contributions and the comprehensive bibliography add to this volume's value.

One of the areas not explored in this collection is that of campaign style—the once distinctively Southern slug-fests where unrestrained name calling and colorful hyperbole provided onlookers with little information but lots of entertainment. Some have suggested that the rise of vicious and vacuous negative television spots actually amounts to the "southernization" of national politics. However, what these two works suggest is that Long's crass language and crude behavior would no longer appeal to voters anywhere—certainly not in today's South, where conservative social and moral values are more fiercely cherished as they appear to be more seriously endangered.

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Environmental Politics and Policy: Theories and Evidence. Edited by James P. Lester. Durham, NC: Duke University Press, 1989. 400p. $55.00 cloth, $22.95 paper.

In this volume James Lester brings together an array of well-known students of environmental politics and policy to both synthesize what has become over the past two decades a rather extensive political science literature on environmental policy making and, perhaps more importantly, to divine the areas fruitful for future research and thought. The effort is at once an assessment of what political scientists have contributed to our understanding of environmental policy making and, at the same time, the use of environmental policy as a lens through which to examine our extant theories about policy-making processes and dynamics. The goal to make "transpolicy" generalizations, Lester explains in the introduction, makes it "necessary to organize the state of knowledge *within* policy areas" (p. 3).

Lester organizes this massive undertaking rather thoughtfully and well, ordering his chapters along an analytical framework of the factors affecting policy formation developed by Richard Hofferbert. According to this framework, policy formation takes place within progressively narrowing boundaries of contextual, institutional and behavioral factors, ranging from the broadest and most evanescent of historical and social contexts to the more specific question of which types of elites sit atop which particular regulatory agencies.

This framework makes analytical sense and provides the reader with a nicely structured tour through the literature and research questions. It also reveals rather dramatically both the areas of knowledge where political scientists have made their greatest contributions and where the discipline tends to fall short compared to other fields of study. It is no surprise, for example, that the volume's strongest and most interesting chapters are those dealing with actors and institutions. Of particular note here are the chapters on interest groups (Helen Ingram and Dean Mann), Congress (Michael Kraft), the bureaucracy (Walter Rosenbaum) and the courts (Lettie McSpadden Wenner), all of which reflect nicely the robustness of the discipline's own general literature on these areas.

A little less successful are those chapters where political scientists seem less comfortable: Henry Caulfield, for example, seeks to examine the evolution of the conservation movement into contemporary environmentalism; but the effort to stake out the broadest of contextual boundaries gets sidetracked somewhat as Caulfield tries to show how particular elites in the Kennedy and Johnson administrations acted as catalysts for and refractors of major attitudinal change. The result is a chapter that could easily have been two separate entities; for the intermixing of the two themes muddies the waters: after all, there are very broad sets of societal beliefs and actions that ebb and flow far beyond the Beltway. Caulfield is not alone within the discipline in his view about the pivotal agenda-setting role played by elites, but the overall effect seems a bit reductionist for a chapter that seeks to set out the broad parameters of policy change.

Similarly, a herculean effort by Stevis, Assetto, and Mumme to synthesize and organize the literature on international environmental politics and policy making reveals how much such terms as *agent* and *structure* have become entangled in ideological and methodological disputes. For example, the authors of this chapter include among their "agents" governments and governmental bureaucracies, a depiction that not a few adherants of pluralism in the U.S. context would regard as the epitome of reification. To their credit, however, the authors underscore the inherent difficulties found in comparative research.

Fortunately for the reader, Dryzek and Lester bring all preceding chapters together into a superbly organized and thought-provoking concluding look at alternative views on the "environmental problematic," developing for the task a taxonomy on environmental worldviews that shows elegantly the position of most political scientists relative to scholars and commentators from other perspectives. Most U.S. political scientists—including the majority of the contributors to this volume—can be considered structural tinkerers, faithful to the basic principles of the constitutional system and confident for the most part that reforming various processes and institutions can enhance environmental protection. Very few political scientists, at least those of the type represented here, take "biocentric" views propounded by "deep ecology" (a worldview dominated by natural scientists) or favor radical reforms, whether in the direction of unfettered free markets or the most severe of Hobbesian command-and-control systems. Political scientists are for the most part believers in the efficacy of well-designed institutions and processes, a bias reflected throughout the chapters.

This volume pursues a noble task. It also is a daunting one, for the sheer scale of what Lester and his colleagues seek to achieve eventually undermines some of the work's overall cohesiveness and punch. Even so, the effort is well worth it. *Environmental Politics and Policy* would be of greatest use in upper-level undergraduate and graduate courses on environmental politics and public policy making generally, though much of what is contained within could also be valuable in graduate seminars on U.S. domestic politics.

CHRISTOPHER J. BOSSO

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Contracting for Property Rights. By Gary D. Libecap. New York: Cambridge University Press, 1989. 132p. $39.50.

A Theory of Property. By Stephen R. Munzer. New York: Cambridge University Press, 1990. 491p. $55.00 cloth, $19.95 paper.

One legacy of the Reagan era has been a questioning of governmental regulatory activity; especially as it affects property rights. Along with this questioning and several Rehnquist Court eminent domain cases suggesting a new sympathy toward these rights, there is emerging a renewed interest among scholars arguing for either a reexamination of property rights in our political-legal culture, construction of new philosophical or economic conceptions of property rights, or a return to conceptions of ownership that were found in nineteenth-century U.S. constitutional law. Among these recent efforts are Libecap's and Munzer's books that approach property rights from contrasting economic and philosophical positions.

Libecap's is a short book employing four case studies that examine efforts by groups or individuals to modify ("contract for") property rights. The author develops in chapter 2 an analytical economic framework explaining the conditions under which property rights will be changed, problems faced when that change occurs, and what actors and institutions are involved. For Libecap, the impetus for changes in property rights is greater as the anticipated aggregate benefits increases, as the compatibility of divergent interests increases, and as the distribution of wealth and knowledge about individual claims under the proposed change is equalized.

The bulk of the book is composed of four chapters applying this analytical framework to changes in federal law concerning mining rights during the gold rush in California and the west in the nineteenth century, land policies in the Pacific Northwest in the nineteenth century, fishing rights off of California in the twentieth century, and 1930s oil field drilling rights in Texas and Oklahoma. These studies lead the author to conclude that past political agreements concerning property rights are important determinates for how future property rights will be created and that the economic context and interests of relevant actors will influence the motivations for change.

In sharp contrast to Libecap's economic analysis of property rights, Munzer provides a philosophical reexamination that seeks to develop a pluralist theory of property. The concern here is not to explain the impetus for change or how it is possible but to address issues such as how we justify property rights, what the limits of these rights are, and how they apply in a real world setting. In arguing against the idea that property should simply be seen in an economic light, the author advocates that property can only be justified and evaluated if it addresses the principles of utility and efficiency, justice and equality, and desert. This pluralist view supposedly allows property to sustain political, moral, and legal justification and be a type of ownership that reflects the multiplicity of demands placed on it in our society.

The book is divided into four sections. The first seeks an understanding of what property rights are, how they are acquired, and how they are related to the individual. This discussion includes a good review of Locke's labor theory of value, Hegel's notion of property as embodied personality, and Hohfeld's concept of property as a "bundle of rights." These discussions offer a nice overview of the philosophical problems associated with each and how the meaning of property has changed over time. The second section moves beyond the relationship of property to the individual and discusses the social context of property and connects it to numerous issues such as Marx's views on property and alienation and the idea of liberty in Republican thought. Part 3 is the core of Munzer's pluralist conception of property and outlines his theory as an alternative to past failures to define and justify property rights. The last section seeks to show an application of his theory to business, taxation, and constitutional eminent domain issues and tell us, for example, whether particular Supreme Court decisions were correctly decided according to his conception of property.

Munzer and Libecap's books are complementary in many ways, and offer some interesting discussions. Libecap's four case studies are often fascinating narratives discussing prospecting claims and conflicts. Similarly, Munzer's book is an excellent review of the history of political theories of property rights along with the particular problems associated with defining, justifying, and translating property rights claims into social practice and rules. But despite many good points, the books are deficient in other ways. They present rival frameworks for an understanding of property rights; but neither adequately addresses the many important issues that the other raises. For example, while Libecap tells us the economic reasons for property rights changes, he ignores the philosophical justification for the institution of property. Munzer addresses the latter but fails to explore the economic and social context that would make likely an adoption of his view of property rights.

Additionally, neither book adequately examines the relationship between property rights and politics. Both books need to place their discussions within the context of U.S. democratic theory or investigate the political (not just the economic or philosophical) foundations on which U.S. conceptions of property are grounded. Libecap, for example, does tell us that property rights are determined by the political process and that political institutions are important to defining the context for their change; but the state is left in the background and out of his discussion. Bringing politics into his economic book would do much to help flesh out his argument and perhaps add an important dimension to his analytical framework. With Munzer, the philosophical emphasis sidesteps politics except as he does seek to develop a legal discussion of property. However, this discussion is philosophical and really does not offer any new insights not previously addressed elsewhere.

Despite these misgivings, political scientists will find both books very readable and important to the current property rights debate.

DAVID SCHULTZ

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The Appeal of Civil Law: A Political-Economic Analysis of Litigation. By Wayne V. McIntosh. Chicago: University of Illinois Press, 1990. 223p. $24.95.

On first perusal this research monograph appears to be a rather mundane case study of the political economy of litigation in one jurisdiction—St. Louis—from 1820 to 1977. On closer inspection, however, it becomes clear that the author's careful, creative use of this 57-year data set has transformed what could have been a rather prosaic case study into an important longitudinal study of the diachronic relationship between socioeconomic-economic conditions and the quantity and quality of civil litigation in an important urban jurisdiction.

To frame his inquiry McIntosh first introduces the reader to the socioeconomic-economic and political history of St. Louis, with special emphasis on key transformations in the locus of economic and political power. He then introduces his longitudinal data base of 4,000 civil cases (250 per year, drawn randomly from the civil docket at 15-year intervals) and uses this sample to describe the evolution of cases and controversies from St. Louis's origins as a booming gateway to the frontier, through the booms and busts of industrialization, to its contemporary postindustrialism. With his study thus framed by the parallel evolution of political economy and litigation, McIntosh's impressive data set enables him to estimate statistically the influence of economic, social, and political transformations on what is litigated, who litigates, how often they litigate, and who wins the legal battles engendered by socioeconomic forces.

The aggregate trends identified by this quantitative analysis constitute a welcome and significant, albeit rather predictable, contribution to the nascent study of what might be termed "judicial economy." McIntosh finds, not surprisingly, that the business of the trial courts tends to mirror commercial and social conflicts that cannot be resolved by other institutions and that the courts' business tends to change in response to changes in social, political, and commercial relationships. For example, prior to the development of the banking industry and the institutionalization of commercial lending, the courts' dockets were dominated by creditors asking the court to help them collect loans from recalcitrant debtors. Nor is it surprising to learn that those who dominate social and commercial relationships tend to prevail in court; that is, the creditors who asked the court for help usually got it, and their victories defined the common law of debtor–creditor relationships.

From an aggregate political economy perspective, McIntosh's quantitative results are neither surprising nor terribly interesting. But the author's analysis transcends this narrow perspective on two important counts. First, he carefully disaggregates his findings in a manner that enables him to creatively incorporate a host of qualitative insights into his quantitative results. This interpretive insightfulness is exemplified by his treatment of divorce cases. After noting the steady increase in divorce litigation since 1910 and offering informed speculation about the effect of postindustrialism on family relations, he shares with the reader some remarkable changes in the nature of divorce disputes. For example, although the percentage of male plaintiffs charging a wife with adultery has dropped from approximately 75% before the turn of the century to only about 30% since 1970, the percentage of female plaintiffs basing their divorce petitions on charges of adultery dropped from 22% in 1855 to less than 10% since 1970. These disparities lead McIntosh to observe that "infidelity has always been a more damaging claim against the wife than against the husband. . . . Such perspectives apparently influenced the legal recourse available to men and women" (p. 107).

The scholarly contribution of this book is further enhanced by the author's ability to incorporate a vast array of qualitative sociolegal scholarship into his analysis and to recognize the implications of his findings for sociolegal models of disputation and litigiousness. For example, in his analysis of the balance between litigation and settlement in the St. Louis courts McIntosh derives his expected findings from the prevailing notions that because trial courts are being overwhelmed by a litigious society, litigants are relying more heavily on negotiation and bargaining to resolve disputes. He finds convincing evidence that the anticipated shift from litigation to negotiation is indeed underway in St. Louis; moreover, this shift is "wholesale and not restricted to any one issue" (p. 124). He also finds, however, that "disaggregating the claims by issue . . . by the kind of plaintiff, . . . by the kind of defendant, . . . and by the relationship existing between the parties consistently yields unique and contrasting patterns. . . . This court has been used in very different ways which current theory does not sufficiently describe or capture" (p. 152).

Thus, McIntosh's careful controls for issue, litigant, and other variables not only subject prevailing theories to a level of systematic empirical analysis conspicuously missing from much sociolegal scholarship, but the resultant analysis also suggests conditional statements that will enhance the sophistication and scope of future models. In combination with his careful attention to the social causes and consequences of his findings, this creative effort transforms his work from a competent, normal science effort into a seminal synthesis of quantitative and qualitative scholarship.

To praise the scope and quality of this monograph is not to suggest that it is without flaws. To the contrary, it evinces many of the imperfections that plague most innovative, ambitious social science efforts. Two persistent limitations are particularly troublesome.

First, although McIntosh purports to be interested in "the role courts play in the development of their surrounding communities" (p. 1), as well as the influence of the surrounding community on the courts, his commitment to a political economy perspective tends to focus our attention on the socioeconomic causes of litigation and to minimize attention to the consequences of litigation for socioeconomic and political development. Were influential, innovative judges instrumental in creating legal environments that helped define the economic, social, and political environments? We don't know.

Second, although the author describes the St. Louis court system briefly for the uninitiated reader, he does not always consider the implications of these legal structures and functions for his analysis. For example, the Missouri Circuit Courts are courts of general jurisdiction that hear civil and criminal cases; yet when the influence of court capacity on rates of civil litigation is estimated, available court capacity is operationalized by reference to civil cases filed per sitting judge, without reference to criminal filings that must be heard by the same judges.

These criticisms are more than quibbles. They limit the scope and constrain the reliability of the analysis. But these limitations do not, by any means, negate the important—in my opinion, seminal—empirical and theoretical contribution McIntosh has made to what he calls the "demand side" of the judicial process. Rather, these criticisms are the product of manageable flaws that can be corrected rather easily in what one hopes will be a long-term commitment by the author and others to understanding the political economy of litigation.

C. K. ROWLAND

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The Money Chase: Congressional Campaign Finance Reform. By David B. Magleby and Candice J. Nelson. Washington: Brookings Institution, 1990. $29.95 cloth, $10.95 paper.

David Magleby and Candice Nelson wrote *The Money Chase* to explain why campaign reform should occur, what the current and probable obstacles to reform are likely to be, what an effective campaign reform package should include, and what the likely electoral impacts of alternative reform measures would be. The authors succeeded in achieving these goals by writing an extremely literate book, one that any educated person can read, understand, and enjoy.

The Money Chase differs from most books concerning money and elections in that it does not attempt to document either the presence or absence of the impact of special interest funds on public policy. The authors assume that contributors, whether individuals or political action committees (PACs), receive special treatment (p. 3). Magleby and Nelson argue, however, that such influence is not the only reason that campaign reform should occur. They justify reform by the fact that legislators spend much of their time chasing money rather than making policy and by the contention that the current system inhibits electoral competition.

The major contribution of *The Money Chase* is its analysis of the incentives that incumbents, challengers, Republicans, and Democrats have for accepting or rejecting various proposals for reform. Persons who cannot understand how Congress can, year after year, fail to reform campaign finance will find in this book answers to their puzzlement. At the same time, however, the authors outline important reasons why it can be in legislators' long-term interests to adopt a package of reforms.

The lesser contribution of the book is its proposed package of campaign finance reforms. As with most proposals, the authors support public funding of congressional elections, expenditure limits, higher limits on individual and party contributions to candidates, a reduced role for PACs, making bundling by PACs illegal, and reducing the costs of campaign advertising through mandated lower rates for broadcast media and postal services. This particular set of proposals is well-grounded in an analysis of how much money it takes a challenger to run a competitive election and is a comprehensive package that provides Democrats and Republicans reasons for supporting the proposals.

The weakness of *The Money Chase* is its failure to examine carefully the question, Why do interests and individuals contribute? The authors provide their brief answer to this question for PACs. They write that although hard evidence of vote buying is rare, PAC contributions provide interest groups access to legislators so that the interest may directly and indirectly affect legislation and define the legislative agenda (p. 196). This omits from group motivations all constituency services and private goods such as those sought by Charles Keating for Lincoln Savings and Loan. Similarly, despite the fact that total individual contributions exceed total PAC contributions in congressional elections, the only information the authors give concerning individuals' motivations is that they give out of self-interest and that "what motivates individuals is little different [from] what motivates most PACs" (p. 52). This failure to examine motivations leads the authors to make recommendations that are inconsistent with their analysis.

When the authors provide their comprehensive package of reforms (p. 200), they propose that individuals should be able to contribute $10 thousand to candidates, $48 thousand to party committees, and $60 thousand in total annual political contributions. If individual motivations are similar to those of PACs, an increase in individual contributions to candidates to the level of current limitations on PACs certainly would not change the influence of special interests. With such high individual limits, the combined contributions of a few members of an organization could have a much greater impact on elections and public policy than current contributions from that organization's PAC.

The authors' willingness to allow such extensive participation by wealthy individuals indicates that the primary goal of the authors is not to reduce the influence of special interests on policy outcomes, it is to increase electoral competition. This is a worthy goal, but many reformers would not agree that the absence of competition is as important as the effect of unequal power between those individuals who can afford to contribute $60 thousand in a year and those who cannot.

KENNETH GODWIN

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The Capacity To Budget. By Allen Schick. Washington: Urban Institute, 1990. 246p. $14.95 paper.

No issue better demonstrates the pervasive governing problems of the U.S. political system than the recurring deadlock over the federal budget. Lurching from crisis to crisis, beset with chronic deficits, policy makers in legislative and executive branches battled along economic, political, and procedural fronts. In this book, Allen Schick assesses the capacity to budget in the United States and, more fundamentally, its implications for the nation's capacity to govern. He does so with an insider's knowledge of budgeting that those familiar with his previous analysis have come to expect. More significant in this work, however, is his broad overview of budgeting and the economy that shows the interrelation of changes in fiscal policy, the composition of the budget, and the procedures by which taxing and spending choices are made.

Evidence of breakdown and erosion are pervasive, Schick argues, but reflect underlying institutional incapacity more than simply the unwillingness of politicians to make tough choices. "Changes in the environment and conduct of budgeting," he asserts, "have weakened the capacity of government to balance resources and demands. If today's politicians are less willful, it is partly because they have less effective tools to do the job" (p. 8). At the root of the problem is the transformation of the budget process from a system of control before World War II to one oriented toward economic growth in the postwar era. When growth sputtered in the 1970s, demands for resources continued to expand while institutional controls to allocate resources weakened. Schick's framework for understanding these changes is centered on three basic components of budgeting: generating, claiming, and rationing resources. The capacity to budget depends on a sufficient balance between these components to produce desired outcomes. Large, persistent, and unwanted deficits, as well as a breakdown in the routines of budgeting, indicate to the author that such a balance no longer exists.

The historical perspective and emphasis on the interrelationship between budgeting and the economy demarcate *The Capacity To Budget* from Schick's numerous other works. Although his concepts of generating, claiming, and rationing resources are not specified as a comprehensive new theory of budgeting, the link to fiscal conditions helps put much of the literature on budget theory of the past quarter century in clearer perspective. He convincingly demonstrates how incrementalism, the preeminent theory of budgeting in the 1960s and 1970s, was an outgrowth of an era dominated by robust economic growth. When fiscal symbiosis turned to fiscal stress, incrementalism ceased to be useful as either a description of, or a prescription for, budgeting. Underlying changes in the composition of the budget solidified claims on resources such as entitlement growth, indexation, tax expenditures, direct loans, and loan guarantees. At the same time, political and institutional changes were reducing the ability of Congress and the presidency to effectively manage these "sticky" expenditures. The ironic result was a budget process that struggled over cutting back while inevitably spending more. Budget incapacity became even more serious with growing paralysis on the revenue side.

When budgeting could not adjust spending demands and revenue capacity to less favorable economic conditions, unwanted deficits resulted and institutions and processes of budgeting begin to unravel. Schick characterizes the results as "improvisational" budgeting in which both Congress and the executive branch abandoned familiar routines and increasingly invented new rules of the game every year. The instability of the congressional budget process continued to mount while the president's budget was transformed from a definitive statement of policy to merely an "opening bid." Schick concludes with suggestions for restoring the capacity to budget, beginning with significant deficit reduction. He advocates more honest accounting, replacing Gramm-Rudman-Hollings, greater control of entitlements, countering the distortion of baselines, strengthening presidential control, recasting the congressional budget resolution, and moving away from budget summits. Even though governing problems stem from more fundamental nonbudgetary factors, restoration of the capacity to budget, he concludes, is an essential part of the overall solution.

The Capacity To Budget is a valuable contribution by one of the leading experts in the field. The book effectively balances detailed analysis with the examination of broad trends in federal budgeting. It is more accessible than Schick's encyclopedic *Congress and Money* published a decade ago and is appropriate for advanced undergraduate or graduate courses. All budget scholars face the danger of the premature obsolescence of their conclusions in today's highly volatile environment. While the dramatic changes rendered in the 1990 budget agreement are missing, Schick's more general conceptual framework for assessing budget capacity minimizes the damage. Earlier chapters, in particular, will withstand the test of time. While his analysis avoids simplistic partisan interpretations or narrow theoretical explanations, the author has his own values and preferences which some may disagree with. A nostalgia for earlier times when budgeting was more routine and dominated by the president seems apparent at times. Schick's discomfort with recent instability in budgeting ignores potentially positive innovations through improvisation, such as the strengthening of congressional leadership that could enhance governing capacity in the legislative branch. In balance, *The Capacity To Budget* reflects the evolution of a mature scholar's thinking about budgeting in the United States and its relationship to how effectively the nation is governed.

LANCE T. LELOUP

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Deterrence and Juvenile Crime: Results from a National Policy Experiment. By Anne L. Schneider. New York: Springer-Verlag, 1990. 127p. $49.00 cloth.

An enormous amount of the governmental policy of interest to citizens (and hence to politicians) is somehow related to crime. This interest displays no party affiliation, surfaces in places and about positions where any connection to crime rates is tenuous at best, and appears immune to the otherwise-devastating critique, "We can't spend our way out of the problem." Thus, we have a highly visible and costly war on drugs, complete with a drug czar, who delights in trashing academic contributions to policy debate. We have a tripling of the incarceration rate over the last decade, caused by a national frustration about crime and violence. In an era of fiscal conservatism, the tax-and-spend attitude of the electorate toward prisons results in the easy passage of public referendums for increased spending authority for state departments of corrections. It even has been argued that the 1988 presidential election turned on the crime issue; the election was placed in jeopardy for Dukakis by images of furloughed convicts and then lost forever the night that he failed on national television to react with outrage and retributive zeal in response to a hypothetical question about the rape of his wife.

In the midst of ever-rising political concern about crime rates and even more rapidly rising public spending comes—in Anne Schneider's new book about juvenile deterrence—some careful, reasoned, and articulate empirical scholarship about the effectiveness of sanctions. This research shows that sanctions that include restitution work as effectively in reducing recidivism as do traditional probation and incarceration programs.

Clearly, such a finding will not find immediate acceptance in policy circles (although restitution is now widespread in adult courts). But the evidence about the relative effectiveness of incarceration is mounting; and when the time is ripe, the cumulative weight of quality empirical scholarship does matter in policy debates about crime, as the demise of the rehabilitative ideal attests.

In the meantime, there is much in this book to recommend it to serious students of crime and justice. Schneider argues that the form of utility theory that underlies modern deterrence theory in the justice system is enhanced by work on decision heuristics and biases. She forges a nice link between these concepts and restorative sanctions. This theme is carried through analyses of data on programs and participants collected to evaluate several new juvenile restitution programs. This integration of theory and program data is refreshing and useful.

Apart from the basic findings stemming from analyses of program outcomes, the book contains a wealth of important data. For example, Schneider undertakes empirical analyses of various recidivism measures and discovers that Lambda—the favorite indicator of the criminal careers movement in criminology—possesses properties that make it highly undesirable. She shows that large-scale, true experiments are possible in operating criminal justice agencies. She studies the relations among perceptions of sanctions, actual experience in the justice system, and recidivism.

Modern deterrence theory in criminal and juvenile justice, made popular recently by the work of political scientists and economists concerned about crime policy, rests on assumptions about criminal and delinquent activity that are difficult to support by data. This form of utility theory suggests that those contemplating criminal acts may be dissuaded by the fear of long-term sanctions, sanctions made more effective with increases in certainty and severity. Schneider's individual-level analyses of the perceptions of the delinquents in her sample show, along with a growing body of research, that it is difficult to show meaningful relations, even in the right direction, between crime and fear of sanctions. If cost-benefit calculations are important in decisions to commit delinquencies, the costs and benefits of the moment are far more influential than those of the far-removed justice system. Schneider's work shows that most sanctions work about equally well, regardless of severity (again, consistent with a large body of research), suggesting that modern efforts to reduce crime by longer and more certain periods of incarceration are wasteful of scarce public resources.

In sum, *Deterrence and Juvenile Crime* is a very useful contribution to scholarship, blending theory-driven analyses with experimental operating agency data, resulting in an important statement about a pressing public policy issue.

MICHAEL R. GOTTFREDSON

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The Party Network: The Robust Organization of the Illinois Republicans. By Mildred A. Schwartz. Madison: University of Wisconsin Press, 1990. 320p. $40.00 cloth, $17.50 paper.

The Party Network is a study of a vigorous, confidently competitive party (no talk of decline and decay); and the author knows her subject well. Political scientists often seem to assume they understand party organizations while only dimly perceiving the full wealth, variety, and richness of relations and experience to be found in them. Schwartz appears to have gained an appreciation of this world; but not too much comes through in this book, which is placed (immolated?) on the altar of rigorous scientific analysis.

The study focuses on some 23 actors whose number includes (as types mostly) county chairs, congressional district and state party leaders, statewide office holders, members of Congress, finance committees, and the Republican national committee. *Party* is defined broadly, and that is basic to the study.

But something is left out. It used to be said and taken as a truism that U.S. political parties were state parties and that within them it was the county parties that mattered most. Not much attention is paid here to the county parties that furnish the base on which all the rest stand. The higher-level people, those with more visibility, higher status in the organization, and some measure of celebrity are studied; and as the book demonstrates, they are now numerous and active. Perhaps the received wisdom (if it is any longer received) should be revised and given a careful restatement.

With regard to method, the author says, "This study is based on 200 informants, interviewed as many as three times and for as long as four hours at a time" (p. 13). Schwartz and her assistants seem to have handled this enormous task very well. Incidentally, structured interviews were prudently avoided.

The crucial part of such an inquiry is to organize or conceptualize the information. Attempts have been made to solve the conceptual problem with regard to party life, but little progress has been made in the last 20 years. David Mayhew's scheme was very useful for his purposes but not a general answer to the conceptual problem. The root of the trouble lies commonly in the mesmerizing influence of the usual behavior science orientation that has plagued the investigators. Cultural anthropology might be the proper relevant discipline.

Schwartz's solution is "to search the organizational literature, looking for any vantage points that could be used to explain the character of political parties" (p. 3). She considers the possibilities of different schools of organizational theory, and, without entirely discarding them, decides to consider the parties as networks of actors that can be described, with the result that what is projected is not a conceptual scheme but a procedure. She adds that "ultimately we have to look to parties themselves (not other organizations) as the source of theoretical generalizations" (p. 16). In other words, study the data to see what concepts and/or "theory" may come to mind.

The principal business of chapter 2 is to present, after describing the party actors, a large sociogram showing the frequency of contacts between types of actors. The succeeding two chapters deal with "network links" and offer an explanation of some of their characteristics in terms of differences in the constituencies of the actors and in competition for the same constituents. Later chapters deal with power, patronage, and ideology. Each is preceded by a discussion of concepts using the literature of sociology and political science.

There is much of interest in these chapters. For example, it is reported that relations among network actors are seldom coercive or heavily sanctioned. Ideological consensus and differences and ideological problems in the organization are handled perceptively. There is much else, but it is not easily summarized. All of it is hard going, not recommended for the layperson despite the interest of the relevant anecdotal material. The language is that of behavioral and political science.

Following a chapter called "Money Talks," which brings the finance committees more clearly into the party net, the author concludes with a discussion of party effectiveness, which, by steps, changes into a good discussion of party adaptation (seen to be a more useful concept than effectiveness). The Republican Party of Illinois is characterized as an adaptive party. What has been considered all along is an already-adapted party that has added to its organization-in-law some additional party and extraparty organizations and strengthened and extended the links between existing party groups. A look back at some earlier time in the party's history would have made that clearer.

Using the older party literature and what I described earlier as a truism in the received wisdom as a base and adding the knowledge supplied in this study and the suggestive material in its last chapter suggests an important conclusion: the older view of parties as county-based and county-centered needs to be recast and changed to include other elements in a much elaborated system, not forgetting the old party, which is still a good starting point. The parties (or one particular party, at least) have been adapting to the age of heavy finance and of candidate and media campaigns. The old, odd, hard-to-describe party has not withered and died or merely lived on as a vestige of the past. That strange creature can adapt and evolve. This is an instructive book as much as for what it attempts as for what it achieves.

THOMAS A. FLINN

Cleveland State University

The Colorado General Assembly. By John A. Straaver. Niwot: University Press of Colorado, 1990. 340p. $27.50.

Some students of state legislatures aspire to build theory or explain structure, behavior, process, and even outputs. They tend to extract specific variables from the rich variety of legislature life, subjecting them to comparative and statistical scrutiny. Other students of state legislatures propose to add to our understanding of one political institution within the context of a single state. They tend to be more descriptive, less quantitative, and intent upon giving the reader a feel for the broader subject under examination. Both types of study, as well as those that fall in between, are necessary if we hope to advance the state of knowledge about politics in the states. The question is not so much the particular approach as how successful the author is in achieving his or her objectives.

This book exemplifies the single-state, institutional, qualitative approach. It grew out of Straaver's 15 years of experience directing a legislative internship program and his desire to better prepare student interns for what they would encounter at the state capitol in Denver. His purpose was simple: "to provide good education for my students and find good entertainment for myself" (p. 328). Anyone who has engaged in the frustrating search for materials with which to orient student interns to a legislature will appreciate the value of his efforts. And anyone who has soaked and poked in a legislature will delight vicariously in the pleasure he derived in the conduct of his research over the years.

Those of us studying state legislatures will learn much about the Colorado General Assembly, a unique political institution. If one's scrutiny is fine enough, of course, every legislature is unique: but then, Colorado's may be more unique than most. It is only one of eight legislatures in the nation where both houses are controlled by Republicans. It is extreme in its low-tax and low-spend ideology. The State of Colorado is surpassed only by New Hampshire in letting local units of government carry the tax burden. The Colorado legislature is one of an endangered species—the citizen legislature, which is being replaced by professional, careerist bodies. Most significant, Colorado has one of the nation's strongest legislatures, even though other legislatures have gained substantially in power vis-à-vis the executive in the last 20 years. The Colorado legislature's power is based in part on the institution's aggressive role in the budget process, through its six-member Joint Budget Committee. It is based in part also in the fact that since 1975 Republican legislatures have been confronting Democratic governors—notably Dick Lamm, who whetted the legislative appetite for combat.

The author's portrayal of the legislature is informative and colorful. His account of legislative leadership, particularly Bev Bledsoe's tenure as speaker in the House, is very illuminating. His descriptions are rich and varied: what the capitol is like on a legislative day; profiles of legislators and lobbyists; a member's social calendar; and an assortment of issues undergoing the legislative process.

Despite the Colorado legislature's individuality in Straaver's analysis, there is much in this volume that strikes a more general chord. Political scientists who advocate a responsible party system can examine one close at hand, including the party caucuses, partisanship, and the assault on party politics by the GAVEL (Give a Vote to Every Legislator) initiative that amended the state constitution in 1988. Political scientists should attend also to the "continuous preoccupation" with reelection (p. 71). Here, as in so many other places, the campaign never ends: legislators starting out their session begin immediately to prepare for the next election. In this respect, as well as some others, states like Colorado and states like California, Illinois, New Jersey, and New York are converging.

The major criticism of this book is that it makes no use of the literature on the subject and thus misses the opportunity to put the Colorado legislature in a larger context. Nonetheless, Straaver has achieved his objectives. He has also furnished us with a book that allows us to compare the legislature in our own states to Colorado's. Recently, Malcolm E. Jewell and Penny M. Miller's *Kentucky Legislature* (1988) and Royce Hansen's *Tribune of the People: The Minnesota Legislature and Its Leadership* (1989) have provided us with valuable works on other states. If others follow their examples, we may have the good fortune to experience a resurgence of legislative studies.

ALAN ROSENTHAL

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COMPARATIVE POLITICS

Political and Social Change in China Since 1978. By Charles Burton. Westport, CT: Greenwood, 1990. 225p. $38.95.

Thirty years of mobilization and "class struggle" campaigns alienated China's citizens, stagnated the economy, and created a severe crisis for the Chinese Communist party (CCP). The dilemma for the post-Mao leadership, according to Burton, was to establish new structures of political authority and new ways of understanding the world that could ensure economic modernization without undermining the CCP. But simply calling it "socialist" modernization does not normatively justify continued party rule. Burton outlines the problem: "The theoretical paradox that the new policies are essential to the long-term continuation of party leadership, but also that they threaten the integrity of the perceived basis for the legitimacy of the Communist party's assumption of state power—Marxism-Leninism and Mao Zedong thought—must be resolved by providing a viable alternative to the perceived 'bourgeois liberalization' tendencies that have started filling the ideological vacuum created by the reforms' effective denial of revolutionary ideals" (pp. 58–59).

Politically, the new leadership sought a balance between Western style, "extreme democracy"—which generates political unrest as individuals and groups ignore collective and state interests—and extreme centralization, which feeds corruption. The leadership's solution, "socialist democracy," as a midpoint between these extremes, generated instability, since no new concrete structures were created for popular political participation.

Ideologically, the CCP faced similar problems in trying to find new ideological forms for "new era" socialism. The new ideology, "socialism with Chinese characteristics," had little practical content as a framework for socialist modernization other than modernizing under CCP rule. While Deng put forward the concept of "seeking truths from facts" to free economic pragmatism from ideological constraints, ideology and the CCP's political interests still determined which "facts" were considered (p. 21). As Burton cogently states, "When 'practise' becomes the 'sole criterion of truth,' theory is not more than propaganda" for continued party rule (p. 78). Similarly, the new moral code of "socialist spiritual civilization" had little meaning except its opposition to "bourgeois liberalization" (Western ideas of liberalism, humanism, and freedom), which rejects party leadership. Party ideologues offered no creative theoretical justification for party leadership of the modernization process, while modernization's demand for decentralizing some political authority threatened the central propaganda apparatus's control and job security of its staff.

The book's greatest strength is to analyze and discuss how all these problems in China's modernization undermined the ideological basis for party rule and generated popular hopes for political change. It helps us understand why the CCP faced student protests against its increasingly corrupt rule first in December 1986 and especially in April–June 1989.

Yet the book has numerous serious flaws. Based on a light revision of Burton's dissertation, the book needed serious editing, especially its style of argumentation, which will lose most readers in the China field. Too many run-on sentences border on the unintelligible, while many interesting arguments needed to be restated in a more succinct, direct form. While the book is intended to address theoretical issues, one stated goal is to show the relationship between economic modernization, political structural reform, and social values. But society and the popular social values that challenge the new ideological and political formulations do not appear in the book. The significant debates over political reform in 1986 among students, intellectuals, factory managers, and party officials in political salons and official and unofficial journals are nowhere to be found, nor is the 1987 battle over "bourgeois liberalization" and the program for political reform of the Thirteenth Party Congress in 1987. For a 1990 book, his discussion is already out of date even before Beijing Spring of 1989.

His discussion of the practical implications of decreased party control is too shallow. While the party has lost some influence in rural party branches and local governments, there is much more secondary information available by a host of Western researchers than one panel on local politics at the Association for Asian Studies Convention in 1986. Most of his Chinese sources are from 1983. If the issue is worth addressing, Burton owes it to his readers to look deeper into the issue.

Finally, the text is very meagre, only 106 pages. Half the book is given over to notes; a glossary of romanized Chinese with unnecessary, albeit beautiful Chinese calligraphy; and a bibliography. While the study indeed adds to our knowledge about the dilemma of ideological reform in the Chinese modernization process, it will be difficult to profit much from it in its present form.

DAVID ZWEIG

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Can Russia Change? The USSR Confronts Global Interdependence. By Walter C. Clemens, Jr. Boston: Unwin Hyman, 1990. 384p. $49.95 cloth, $19.95 paper.

The USSR and the Western Alliance. By Robbin F. Laird and Susan L. Clark. Boston: Unwin Hyman, 1990. 269p. $44.95.

Both of these books examine recent developments in Soviet foreign policy and their relationship to past Soviet policy successes and failures. The similarity between the two begins and ends there. Indeed, it is striking how books on roughly the same subject and written at approximately the same time can approach their material in such radically different ways and reach such radically different conclusions. The differences reflect not only a variation in their respective views on the USSR but, more fundamentally, a different approach to, and understanding of, international relations.

The more ambitious of the two books, in terms of scope and agenda, is that of Clemens. Even his broadly inclusive title, *Can Russia Change?*, fails to reflect the breadth of his concerns; for Clemens is interested not only in change in Soviet policy but in the possibility of change in the character of international relations in general. Specifically, Clemens examines the possibility of movement away from the zero-sum, realist paradigm that has long dominated both scholarship and policy making toward a new globalism based on mutuality of interests in an interdependent world. While such challenges to realism are hardly new to the literature on international relations, Clemens's contribution is to link that theoretical debate to the more concrete issue of change in Soviet foreign policy and East-West relations under Gorbachev. To the extent that the USSR, once arguably the most "realist" of states, now appears, as Clemens suggests, to be embracing the new globalist approach to international relations, the case for movement beyond the realist paradigm becomes ever more difficult to dismiss.

In contrast to Clemens, Laird and Clark address a much narrower set of concerns. Their book examines the evolution of Soviet perspectives on the NATO alliance. It should be noted that while Laird and Clark dominate the book (responsible individually or as a team for 6 of the 10 chapters), there are also contributions by Philip Petersen and Notra Trulock III, Hannes Adomeit, Charles Gati, and Erik Hoffmann. From different vantage points, each of the contributors assesses the character and effectiveness of the traditional anticoalition strategy pursued by the USSR toward the West. Despite differences of emphasis among the various contributors, the analysis as a whole sharply contrasts with that of Clemens.

First, while recognizing change in Gorbachev's foreign policy, the Laird and Clark volume views those changes in terms that are qualitatively different from Clemens. While Clemens interprets Gorbachev's "new political thinking" as a revolutionary paradigmatic breakthrough, the Laird and Clark volume tends to view Gorbachev's foreign policy revisionism in less sweeping terms. Petersen and Trulock suggest that the Soviet debate over the role of military power in national security has had only a "marginal impact" on operational planning (p. 31). Hoffmann concludes that the traditional anticoalition strategy has "been modified but has not changed dramatically" under Gorbachev (p. 81). Laird argues that the most significant change in Soviet strategy under Gorbachev has been in "the public diplomacy arena" (p. 1). In short, the book tends to view Gorbachev's foreign policy reforms largely as a matter of shifting tactics designed to pursue more effectively the traditional Soviet goal of weakening the Western alliance.

Second, the two books draw different lessons for Western policy makers. Whereas Clemens interprets the Gorbachev reforms as an opportunity for the West to join its former Cold War adversary in a quest to find an escape from the zero-sum world of international politics inherited from the past, the Laird and Clark book sees the foreign policy of Gorbachev as a challenge that the West must meet lest we "leave Gorbachev in a position to manipulate the current phase of East-West relations decisively to Soviet advantage" (p. 255). These divergent recommendations reflect the fact that while Clemens is quite openly convinced of the need to transcend realism (quite apart from the question of Soviet thinking along these lines), the Laird and Clark volume, in a less overt way, utilizes the assumptions of that paradigm as the foundation for its own analyses.

Despite their differences, the two books both have much to recommend them. Each is well researched, making ample use of both Soviet and Western sources. While it is hardly possible, given their differences, to accept and endorse in full the conclusions of both, readers will find plenty of food for thought in the two volumes. At the same time, each book does have its weaknesses.

The central problem with the Laird and Clark volume is that it has already been dated by events. Instant obsolescence is, of course, an occupational hazard in this era of rapid change. To their credit, Laird and Clark have adopted the useful practice of beginning each chapter with a footnote indicating the month when research was completed. With the curious exception of Gati's chapter on Eastern Europe (which was completed back in May 1987), most of the chapters in the volume take account of events up through the end of 1988. While they all provide useful background to the most recent developments, one might be better served by waiting for more current analyses by this group of generally prolific writers. It will also be interesting to see whether and how the most recent developments in Europe affect the interpretation of events offered in the book.

Given its broader and more theoretical focus, Clemens' book is less directly affected by the problem of timeliness. Indeed, even those inclined to disagree with his thesis would find it very easy to anticipate how he would fit the most recent developments in Soviet policy toward Europe and the Persian Gulf into his argument. On the other hand, the book seems to be overwritten in places and suffers from some organizational lapses. For example, in a chapter in which Clemens uses developments of the 1980s to test a number of conventionally accepted principles of arms control, the reader is forced repeatedly to flip back and forth between the list of principles at the beginning of the chapter and the analysis that follows. These relatively minor problems aside, the book should be of interest not only to students of Soviet affairs but to specialists in international relations concerned with the realist-globalist debate.

RUSSELL BOVA

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From Zarathustra to Khomeini: Populism and Dissent in Modern Iran. By Manochehr Dorraj. Boulder: Lynne Rienner, 1990. 220p. $28.50.

The State, Bureaucracy, and Revolution in Modern Iran: Agrarian Reform and Regime Politics. By Ali Farazmand. New York: Praeger, 1989. 289p. $42.95.

Iran: From Royal Dictatorship to Theocracy. By Mohammed Amjad. New York: Greenwood, 1989. 178p. $39.95.

Iran's Islamic revolution has renewed the classic debate on the role of ideology versus political economy in explaining the revolution. In *States and Social Revolutions* (1979), Skocpol clearly opted for a structural perspective, denying the explanatory value of ideology. However, in her article on the Iranian Revolution in *Theory and Society* (1982), she came to note the central role ideology played in it. Later, in response to a substantial critique by William Sewell concerning the role of ideology in the French Revolution, Skocpol qualified her earlier statements, saying that "patterns of culture and ideology are causally unique to each revolution." Iran's Islamic revolution clearly fits that designation. Indeed, Islamic thinkers like the late Mutahhari view the Islamic revolution as unique among modern revolutions in that it is above all an "ideological movement" (a phrase enshrined in the Constitution of the Islamic Republic). From this perspective, socioeconomic and political factors, although important in bringing about the regime crisis, cannot explain the nature of the revolution and emergence of the Islamic republic. Their common view is that Western cultural domination transformed the historical tension between ideals and social realities of Islamic society into a fundamental contradiction that could be resolved only through revolution. These three books, like most studies of the Islamic revolution, adopt a structural perspective and even avoid the term *Islamic revolution.*

In *Iran: From Royal Dictatorship to Theocracy* Amjad brings much relevant information to bear on his thesis that the historical weakness of the bourgeoisie and the dominant role of the state allowed the Pahlavi regime to assume a central role in the integration of Iran into the capitalist world economy, a condition that further strengthened the role of the state against civil society, especially following the 1962 Land Reform. The shah's exclusive reliance on oil, the state-dependent bourgeoisie, and the state bureaucracy to run the economy led to destruction of agriculture and the uprooting of the peasantry, the disaffection of the Bazaar merchants, and unfulfilled expectations of the middle class. In the absence of political institutions, the shah's response to disaffections was control and repression. Amjad's explanation of the Islamic character of the revolution is that the weakness of class formation, "pervasiveness of the petty bourgeoisie," and "lack of political education" allowed the religious leadership to take control of the uprising and to impose the Islamic ideology (p. 122). Defining revolutions as transformations of state and class structure through class-based action, he concludes that the Iranian Revolution was a political, rather than a social, revolution. Could Islam have its own conception of social transformation? Thus Amjad explains the revolution by its "shortcomings," looking at parallels rather than the realities of the Iranian Revolution. Although he judiciously avoids the term *feudalism* to denote Iran's past agrarian structure, when it comes to modern Iran he freely employs concepts and categories derived from the experience of Western societies. Amjad contends that Ayatollah Khomeini announced the aim of creating an Islamic state after the overthrow of the monarch (p. 137), a claim that cannot withstand a scrutiny of Khomeini's Paris proclamations, interviews, and revolutionary slogans. Despite its general theoretical misconceptions, Amjad's work is an informed and useful contribution.

An important concept that can enable us to relate ideology to structures and regime policies and bridge the rift between ideological and structural approaches to revolution is the concept of legitimacy. The main slogan of the Iranian Revolution—"Independence, freedom, Islamic Government"—profoundly questioned the legitimacy of the regime. When did the shah's regime lose its legitimacy? Farazmand gives serious attention to this important question. After the 1953 coup, there developed a shared sense between the shah and most Iranians that the shah's regime was a U.S. creation and did not represent their independence. Without a significant domestic civilian foundation, the shah, aided and advised by the United States, sought to overhaul the Iranian economy in industry as well as agriculture in a way that would depend primarily on external, especially U.S., sources and expertise and associated groups such as comprador bourgeoisie, Jewish and Bahai elites, and the Westernized classes. The shah could rule Iran only as a "virtual colony of the United States" (pp. 18, 169).

The land reform program removed the most formidable obstacle to the shah's strategy of subjugating Iran to the new political economy: a self-sufficient agriculture and a potential source of rebellion. Agrarian bureaucracy and agricorporations became instruments for extension of neocolonialism and state control into rural Iran, resulting in destruction of agriculture and migration of millions of peasants to the cities. The shah would undermine what he could not control.

Yet because Farazmand severs the problems of dependence and dictatorship from the cultural context, he fails to note the Islamic origins of the Pahlavis' illegitimacy in their attempts to supplant Islam and the *ulama* in Iran. He perceptively notes Khomeini's condemnation of the shah's violations of the 1906 Constitution (which gave the religious leaders the right to judge the Islamicity of legislations) and the religious leaders' opposition to "cultural subordination of Iran" to the West (pp. 34-35). Farazmand's analysis of the Iranian state and its agrarian bureaucracy is excellent. He writes with an intimate knowledge of the Iranian bureaucracy, a native familiarity with the problems of rural Iran, and a mastery of most theoretical and empirical issues he discusses. The book also provides important insights into the origins of the revolution. Farazmand does mistakenly refer to Iran's agrarian structure as "feudalism" throughout and to the shah's land reform as a transformation that "produced some benefits to the society" (p. 62). There is nothing in the book to substantiate either except a latent assumption about a universal pattern of historical change. The name of the parliament is Islamic (not National) Consultative Assembly, and the Islamic Republican party ceased to exist in 1987.

Whereas Amjad and Farazmand use class and state formation to explain the revolution, Dorraj employs the concept of "populism," purportedly to reconcile structure and culture and to avoid Marxist and Islamic reductionisms. Yet his "Islamic populism" is dormant ideological formation, "the mark of an earlier stage of development" (p. 171) reactivated in response to dislocations of modernization. Iran's Islamic upheaval, in his view, was the sigh of an oppressed and uprooted peasantry and lower social classes yearning for security, justice, community, and a return to folk traditions (populism), a condition that made these groups "vulnerable" to Islamic "peasant ideology" (p. 170). The ideology was provided by the clergy, "an archaic social sector whose livelihood and existence was threatened" by modernization and therefore "sought to protect itself with a thick layer of mass support by mobilizing the masses" (p. 175). Dorraj finds the immediate root of clerical populism in the neopopulist orientations and ideas of 1960s and 1970s in which he includes the literary circles, Marxist guerilla groups, inconsequential writers (chaps. 8-10) as well as the teachings of the Islamic thinker, Ali Shariati (chap. 10). In reducing the corpus of Islamic ideology to a few populist concepts and conflating vastly diverse intellectual and political trends, Dorraj constructs two competing populisms, with the clerical populism as the winner due to its mass following. Thus, he says, the revolution that began with the activities of the modern intelligentsia "culminated in seizure of power by the fundamentalist clergy" (p. 175). What is known beyond conjecture is that the Islamic movement led by the *ulama* has its own distinct intellectual and political history, originating, in most recent times, in the religious renewal movement founded by the Mutahhari in 1960 and in the religio-political uprising led by Ayatollah Khomeini in 1963. Shariati was a product of this religio-political movement and advanced its interests. The revolution did not begin with poetry readings and other acts of secular intelligentsia but with clashes between Qom theological students and the police in June 1978. The most effective participants in the revolution were the civil service and private sector employees, the bazaar shopkeepers and merchants, and Islamic students and intellectuals, not the rural migrants. Also, the idea of "clergy's vested interest" (p. 161) adopted by the Western observer of Islam obscures our knowledge because the *ulama*'s interest in Islamic teaching has always been regarded as public interest, hence their "deep social roots among the people" (Dorraj, p. 159). A significant portion of Dorraj's book is devoted to finding the common roots of Irano-Islamic populism in Zoroastrianism as well as Islam and in Shi'ism as well as the syncretic movements in the early Islamic Iran, namely, in vastly different traditions with profound differences in their concept of justice, community, and the good. Dorraj's views of Islam as modified Zoroastrianism and of Shi'ism as "Persianized Islam" lack historical foundation, but they do represent a significant intellectual tradition in recent Iranian history, which Dorraj presents with knowledge and eloquence. But in order to understand the Islamic revolution, we must peek into the subterranean world of Iranian society, especially during the 1960s and 1970s. That world was ruled neither by the state nor by the Marxist groups or the secular intelligentsia. It was being reconstructed by the Islamic ideology and leadership.

ABBAS NAVABI

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Social Security Policies in Industrial Countries: A Comparative Analysis. By Margaret S. Gordon. Cambridge: Cambridge University Press, 1988. 377p. $49.50.

This long and abundantly detailed book provides a systematic comparison of the major types of social security policies in 28 industrial countries. Following a general survey of postwar developments and variations in spending patterns, Gordon examines pension and retirement programs, disability and industrial accident policies, health benefits, unemployment compensation, family policies, and public assistance. This focus captures major elements of the welfare state but excludes housing, in-kind services, and education. Gordon concentrates on particular policy areas rather than on an analysis of the welfare state in general. She considers the role of international organizations like the International Labor Office, the Council of Europe, and the European Economic Community and concludes with some general observations about current issues.

Gordon notes that much previous work has concentrated on the general shape of social security policy and has neglected the details. Gordon's work aims to remedy this situation. Her command of the intricacies of policy is extraordinary. For most issues she provides the pertinent history (particularly for the period since 1945), the scope of coverage, financing mechanisms, and the structure of benefits. Rather than grant each country equal weight, Gordon centers her attention on the nations that have adopted programs that have served as a model for others. Gordon considers among other things the varying impact of earnings-related, income-tested, and universal benefits; the need for vigorous labor market policies; the adequacy and equity of benefits; the distribution of financial burdens; and the broad variation in family policy, public assistance, and disability coverage from country to country.

There is no talk of a general crisis of the welfare state here. Gordon discerns steady progress in the expansion and improvement of programs into the 1970s, at which point straitened economic circumstances, severe inflation, family instability, increasing unemployment especially among the young, and homelessness posed new difficulties. Gordon clearly admires the advanced welfare states and laments U.S. shortcomings in such areas as health care and family policy.

It would be nice if a scholar so conversant with the details of programs were more ready to generalize, if her mastery of empirical niceties yielded more theoretical results, and if her comparison of parts led to comparisons of whole systems, particularly since parts are not always appropriate units of comparison. (Income support systems, for example, may include unemployment insurance, aid to the children of the unemployed, means-tested housing benefits, and food stamps, all of which contribute to replace lost earnings.) When Gordon does choose to generalize and speculate, the results are provocative. In a stimulating final chapter she diagnoses emerging problems. She foresees increasing attacks on earnings-related pensions and other programs for failing to target the poor more efficiently. She urges more cooperation between schools and labor market authorities in order to combat youth unemployment effectively. She stresses the importance of rehabilitating the disabled both to reduce dependency and to alleviate possible labor shortages. She notes that the rapid expansion of medical care costs has not produced commensurate gains in health and endorses greater concentration on preventive measures, not least in work places. Finally, she recommends attention be given to the fit between income tax rates and social insurance contributions with an eye to making the financing of social security more progressive. Greater willingness to formulate observations of this sort would have made this book more valuable as a monograph or a text; as it stands, it is not a major theoretical contribution but a valuable reference on social security programs in the mid-1980s.

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Book Reviews: Comparative Politics

China and the Challenge of the Future: Changing Political Patterns. By Carol Lee Hamrin. Boulder: Westview, 1990. 257p. $18.50.

Behind the Tiananmen Massacre: Social, Political, and Economic Ferment in China. By Chu-yuan Cheng. Boulder: Westview, 1990. 256p. $29.95.

China's Crisis: Dilemmas of Reform and Prospects for Democracy. By Andrew J. Nathan. New York: Columbia University Press, 1990. 242p. $24.50.

The bloody Tiananmen Incident of 3–4 June 1989 and the massive popular demonstrations that preceded it caught most China analysts by complete surprise. They had totally underestimated the extent of the contradiction that had emerged in China between major social and economic change and relative political obduracy. More than 18 months later, there is still a need to better understand the meaning of the reform decade and the true nature of the crisis faced by China today. The books under review provide an important contribution to this effort. Each presents a somewhat different perspective on the meaning of China's dilemma.

In *Behind the Tiananmen Massacre,* Chu-yuan Cheng adopts a sweeping, historical approach, describing the long-term trends that led to Tiananmen and their general pattern of interaction: popular discontent fueled by growing social and economic malaise; the demonstration effect produced by a decade of close contact between Chinese intellectuals and students and the industrial democracies; the rigidity and personal, power-oriented nature of the one-party Leninist system; and the escalating succession struggle within the top leadership.

Cheng argues that the 4 June massacre was "completely unnecessary and unjustified," has halted economic reform, destroyed Deng's painstakingly constructed succession arrangements (thus posing long-term losses for China's modernization drive), and created an untenable situation for the current hardline regime. The death of Deng Xiaoping and his elder conservative colleagues and the worsening of the economic crisis will most likely "pave the way for the resurgence of the reformers" (p. 11).

Little to argue with here. For all its merits as a general overview, however, there is a disturbing quality about Cheng's book. The analysis is a little too neat and simple and fails to convey a sense of the complexities and ambiguities that produced Tiananmen and continue to influence developments in China today.

Hardline party conservatives are caricatures of a sort—nay-sayers united in their opposition to change. Little attempt is made to define their views and motivations and assess their differences. The People's Liberation Army (PLA) is also viewed as a bulwark of reactionary dogma that consistently blocks all reforms and is apparently able to stand apart from and above the civilian leadership and ultimately "decide" what role it shall play in the future. Yet reform has actually had a major impact on PLA attitudes and personnel, creating serious internal divisions and unrest, which casts doubt on Cheng's view that the military as an institution is poised in the wings, ready to determine China's fate.

In contrast to the breadth and simplicity of Cheng's study, Carol Lee Hamrin provides a more complex, microscopic dissection of the twists and turns in China's reform policies. Relying largely on interviews with key participants, Hamrin provides an inside look at the beliefs, organizations, and tactics of the reformist leaders and their expert advisers as they struggled to define their program and counter the attempts of conservatives to dilute or reverse many of its essential elements.

While acknowledging some major limitations (e.g., a focus on the years 1983–87 and a concern with policy formulation, not implementation), the author suggests that the book contributes to our general understanding of the policy process, particularly in its attention to the relationship between leaders and bureaucracies and the increasingly important role played by specialists in influencing the formulation and refinement of the reform program. Yet as a whole, the study has the ring of intellectual history more than political analysis of leadership struggle and decision making. The evolution of the reform debate and the clash of schools of thought receives prominent place in most of the chapters, not the actual political maneuvering of individuals or groups.

Nevertheless, the study offers much of value in understanding both the reform period and the situation facing China today. The issues basic to the ongoing debate over development strategies are presented with unprecedented clarity. The book also shows how the reforms produced a major, albeit often subtle, change in many of the perceptions, values and procedures basic to Chinese politics and society, beginning a "process of transition to a more open politics" (p. 210) that undermined the ability of the Chinese communist system to survive the demise of its original leadership, even without the occurrence of the Tiananmen incident.

Yet Hamrin also recognizes that basic political patterns of the reform period remain linked to both the Leninist political structure (the "chief weakness in the 1980s reform program") and the political culture of dependency and continue as an obstacle today. As a result, for Hamrin, "the potential for serious economic and social crisis and systemic collapse before the next century dawns is now far greater than the prospects for an economic and social miracle" (p. 223).

China's Crisis is largely a collection of Andrew Nathan's past essays (many abridged) on social and political change in China and Taiwan. In general, I am not terribly fond of compilations of past writings, especially when their reappearance is occasioned by some highly public event (in this case Tiananmen). It creates the suspicion that commercial motivations may have taken precedence over a genuine desire for relevancy. My suspicions were not entirely allayed by the Nathan volume. For example, I am not sure how chapter 8 (a detailed article, written in collaboration with Yangsun Chou, on aspects of the democratic transition in Taiwan, first published in March 1987) adds to our understanding of China's current crisis. Nathan himself states in the essay that the Taiwan experience "carries no easy lessons for the mainland" (p. 151).

Despite such shortcomings, however, the book retains considerable value to the specialist and generalist alike. Nathan's most interesting observations result from his exploration of the key question facing China today: the possibility of a peaceful transition to a democratic society. This is presented largely in the recently penned conclusion of the volume, not in the essays. Nathan concisely summarizes the nature of China's dilemma: "All the problems that brought on [Tiananmen] are still there and are worse than before; the means to deal with them are weaker than before" (p. 207). Such a situation tempts an authoritarian solution. But as Nathan points out, the most such an authoritarian regime can achieve in the current context is repression, which would rely for its ongoing effectiveness on the constant loyalty of the Chinese military, a dubious proposition.

Given such dire straits, and the continued significance of factionalism within the leadership, to Nathan it is quite probable that "some groups within the regime will see sponsoring change in the direction of democracy as a way of improving their position against other factions" (p. 207). But Nathan harbors no illusions about the difficulties entailed in making a peaceful transition from a weak, conservative regime fearful of its own population to a workable participatory democracy. He notes that in a post-Deng environment, the Chinese military could help the situation by shifting from being a supporter of repression to a guarantor of ultimate order during a transition that would likely prove to be "hard, prolonged, complex—and inconclusive," involving a shaky mixture of democratic and authoritarian elements, at best. Of course, this scenario relies to a great extent on both the continued unity of the military and the continued patience and nonviolence of the democratic opposition, as Nathan points out. For me, such a recognition cannot inspire great confidence in China's future.

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Culture Shift in Advanced Industrial Society. By Ronald Inglehart. Princeton: Princeton University Press, 1990. 484p. $49.50 cloth, $16.95 paper.

Culture, Structure, or Choice? Essays in the Interpretation of the British Experience. By Paul V. Warwick. New York: Agathon, 1990. 251p. $36.00.

Ever since the concept of political culture became an important component of comparative politics, it has been the subject of controversy. A considerable body of survey research has revealed both cross-national and intranational attitudinal differences but has often failed to demonstrate clearly the explanatory or predictive value of cultural factors. Anthrohistorical approaches have been extremely suggestive but frequently use culture as a residual variable to explain everything that structural factors cannot.

Drawing upon the Euro-Barometers and other Western European survey results from recent decades, Ronald Inglehart continues his long-term examination of postmaterialist culture in advanced industrial societies. Having enjoyed relative economic security during their formative years, postmaterialists, he argues, are less preoccupied than are materialists with personal economic advancement and more politically concerned with issues such as the environment and disarmament. Rather than having produced a generation of self-centered yuppies, postwar affluence, he insists, has created the electoral base for the Greens and other socially concerned movements. Since better-educated, more affluent voters form the core of the postmaterialist subculture, the longstanding inverse relationship between income and support for left-of-center parties has been diminished, if not reversed, in Western Europe.

The book's major contribution is its outstanding body of survey data on the postmaterialist "culture shift." Inglehart's arguments are well known, and he presents little here that is new. Rather, his goal is to demonstrate that his hypotheses, first offered in the early 1970s, have not only stood the test of time but have been further validated by new data sets and longitudinal analysis.

Inglehart's thesis is clearly valuable and points to new lines of political cleavage and party alignment in the coming years. However, the book tends to overstate the significance of culture shift. Economic security may not be as entrenched among postwar generations as Inglehart leads us to believe. During the stagflation of the late 1970s and early 1980s, for example, many young voters who had enjoyed economically secure formulative years turned to Thatcher, Reagan, and Kohl, presumably more concerned with economic growth than ecology or disarmament or other postmaterialist issues. Class and economic divisions may be diminishing as explanatory variables, but it was Thatcher's poll tax, not her ecological or defense records, that severely eroded her popularity in 1989–90. Similarly, Inglehart's expectation that "relatively high levels of job security [would] . . . give rise to an increasingly Postmaterialistic outlook" in the Soviet Union (p. 429) demonstrates the danger of looking too hard for universal trends.

At times, I wondered about the validity of Inglehart's indicators. His attempt to explain the inverse relationship between national suicide rates and measurements of "happiness" and "overall life satisfaction" (chap. 7) are not satisfying. Here, and in other areas, within-nation analysis would have been helpful. At the same time, the data presented confirm Inglehart's hypotheses with monotonous regularity, while the effects of postmaterialism are usually demonstrated with "a cross-national consistency that is almost breathtaking" (p. 135). The book might have benefited from greater discussion of cross-national differences and analyses of deviant cases. These criticisms notwithstanding, no serious student of industrial democracy should fail to read this book.

Because Paul Warwick's research is historically based, he lacks the precise cultural data available to Inglehart. Looking for the causes of Britain's rise and decline as an economic power, he examines contending hypotheses in the literature, using a "strong-inference" approach (p. vii). His questions are simple yet obviously relevant to far more than an understanding of Britain: What caused Britain to take the lead in the industrial revolution? What caused its economic decline, beginning in the early nineteenth century? Why has the British class structure and political system retained its legitimacy? Drawing on an impressive range of historical studies, Warwick contrasts the explanations of *culturalist, structuralist,* and *rational choice* models.

He begins by examining the works of Weber, Wallerstein, and others, seeking to explain the rise of capitalism in particular loci. He then examines the roots of Britain's economic decline. Cultural values, he argues, do indeed play a role at both ends of the British rise and fall. Thus, British industrialization lost its energy when the bourgeoisie abandoned its competitive capitalist ethic and adopted the "aristocratic values [of the landed gentry and the imperial ruling class] of honor, duty and disdain for industry" (p. 202). Rather than present new evidence, the book tests and synthesizes the work of leading scholars on each of the periods. Warwick continually tantalizes us with exquisitely elegant explanatory theories, which he then, just as the reader is convinced, proceeds to refute with well-fashioned counterarguments.

Ultimately, he finds strengths—but also important weaknesses—in all three approaches. The culturalists lack causal specificity. Structuralists are unwilling to admit that "values can survive the structures that gave them birth" and are often unable (the Marxist theory of class struggle being the exception) to explain what causes structures in the first place (p. 205). Rational choice theorists avoid the "squishiness" of culturalism by excluding norms and values, erroneously insisting that all decisions are based on the individual's short-term material advantage (pp. 174–75).

If Inglehart's work suffers from a certain repetitiveness and predictability (one can bet that postmaterialism will explain the dependent variable), Warwick continually surprises us by taking his argument through new turns. Perhaps the greatest weakness of Warwick's book is that most readers must frequently accept its arguments on faith, since only economic historians are capable of questioning many of Warwick's inferences (and because his own arguments are sometimes "squishy"). This, however, is a small price to pay for what is truly an intellectual tour de force. Even the reader who doesn't agree with all of Warwick's conclusions can't fail to recognize what a tremendous contribution this book makes to the study of culture and political-economic change.

HOWARD HANDELMAN

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The Soviet Concept of "Limited Sovereignty" from Lenin to Gorbachev: The Brezhnev Doctrine. By Robert A. Jones. New York: St. Martin's, 1990. 337p. $45.00.

What does it mean to have "socialist interstate relations?" To the extent that the very concept of sovereignty is rooted in a capitalist and legal positivist order, how have the foreign policies of the Soviet Union and its socialist neighbors been reconciled with international law? This book is a history of the Soviet concept of relations between sovereign socialist states. Originally published in 1989, it ends just before the collapse of the old order in Eastern Europe; but that makes the study no less valuable.

The book focuses on the management of the East European "Soviet bloc," but always with an eye to its historical and philosophical context. An extensive review of Soviet legal and philosophical literature demonstrates that the Brezhnev Doctrine "in no sense represented a qualitative shift in Soviet attitudes towards the concept of sovereignty" (p. 257). Instead, postwar theories of socialist internationalist relations can be traced to ideas developed in the interwar period. What had changed between 1920 and 1968 was that the old "surgical interventions" were justified as the means of giving birth to socialism, while the Brezhnev Doctrine was intended to abort the birth of challenges to the Soviet model and Soviet power. Since 1968 the "two decades following Prague Spring have demonstrated that, in responding to the challenges to bloc cohesion, the Kremlin has been vainly struggling against a hydra-headed monster" (p. 237).

Jones demonstrates a good grasp of the current literature in international relations and comparative politics and uses the case of the Brezhnev Doctrine to explore the nature and limits of ideology, the differences in the doctrines of intervention of both the United States and USSR, the interaction between internal nationalities policy and foreign relations, and the impact of global interdependence on Soviet policy. He moves beyond merely showing the evolution of policy and uses the concept of sovereignty to explore "the Soviet Union's essentially dualistic, or 'bi-axial' perspective on international relations" (p. 17) in which the "horizontal" axis is based upon "legal positivist" conceptions of the principles of international law and the "verticalist" axis is based upon "proletarian internationalism" and Marxism-Leninism. The interaction of these ideas has produced a "transition from the attitude of rejection, to cynical acceptance, and then one of obsessive reverence for the sovereignty principle" that has "coincided with the transformation of the Soviet Union's role within the world system" (p. 27). Today, Jones suggests, we may be witnessing the rise of a third axis: the "new thinking" of the Gorbachev era, based upon a fear of technological backwardness and a recognition of global interdependence.

This book ends soon after the Soviet withdrawal from Afghanistan and does not anticipate the rapid changes of 1989–90. In fact, like most Soviet specialists of the time, Jones cautions against expecting too much too soon. There is no mention of the "Sinatra Doctrine." A few statements, such as Jones's estimate of Stalin's capability formally to have incorporated much more European territory into the postwar USSR, are more stimulating than convincing. But to Jones's credit, recent events do not make this book obsolete. If anything, they make it more valuable. It is a useful analysis of the contradictions within the concept of socialist sovereignty and between the formal definition of sovereignty and the informal system of control. It shows why these contradictions have become more and more difficult to manage and provides a guide to understanding how these contradictions continue to contribute to the transformation of the Soviet bloc.

DANIEL MCINTOSH

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Continental Divide: The Values and Institutions of the United States and Canada. By Seymour Martin Lipset. New York: Routledge, 1990. 337p. $29.95.

Culturalist approaches to politics, enormously popular in the 1960s, became virtually moribund the following decade after suffering ideological attacks from the Left and methodological attacks from the Right. Even the concept of political culture seemed doomed. Early reports of its demise proved premature, however. Concern with language and discourse opened up new ways of conceptualizing culture. Some students of development reiterated the significance of culture. Several Marxist scholars questioned the canon that culture is mere superstructure. There was even revived interest in the pioneering work of Louis Hartz.

Although many scholars abandoned culturalist approaches to politics in the 1970s, Seymour Martin Lipset did not. This book is the culmination of his work (begun over 40 years ago) on the comparative analysis of the values and institutions of Canada and the United States.

Lipset acknowledges that these nations "resemble each other more than either resembles any other nation" (p. 212). Yet this is precisely why each yields rich insights about the other. The many features they share make it easier "to isolate the factors responsible for differences between them" (p. xiii). Notwithstanding numerous structural differences between Canada and the United States, Lipset contends, the key factors are cultural: "I try to interpret aspects of the North American cultures as reflections of their key organizing principles derived from their varying histories and ecologies" (pp. 7–8).

Initially drawn to Canada to study North America's first elected socialist government (*Agrarian Socialism* [1950]), Lipset first presented systematic comparisons of Canada and the United States in *The First New Nation* (1963). This earlier work involved a four-country comparison (with Australia and Britain) and featured as its theoretical framework a modified version of Talcott Parson's pattern variables. Lipset dropped Britain and Australia in *Revolution and Counterrevolution* (1968) and has generally moved away from Parsons, who does not even appear in the index or bibliography of this book.

Still prominent—indeed, it serves as his point of departure—is an inchoate theory of "formative events" rooted in Max Weber's analogy of "loaded dice." "[As, in] a dice game in which each time the dice come up with a certain number, they are loaded toward coming up with that number again, [so] current differences among social structures can often be linked to specific historical events that set a particular process in motion in one nation or unit and a different one in a second" (p. 16).

The key formative event for the United States is obviously the War of Independence. For Canada it is the counterrevolution:

> Canada has been and is a more class-aware; elitist, law abiding, statist, collectivity-oriented, and particularist (group-oriented) society than the United States. These fundamental distinctions stem in large part from the American Revolution and the diverse social and environmental ecologies flowing from the division of British North America. The social effects of this separation were then reinforced by variations in literature, religious traditions, political and legal institutions, and socioeconomic structures" (p. 8).

To substantiate this thesis Lipset discusses and analyzes a dazzling array of data (surveys, demographic statistics, social indicators, political institutions and practices, economic variables, historical accounts) as they illuminate differences and similarities in law; religion; the economy; ethnic, gender, and minority relations; center-periphery relations; public policy; and so on. Over the years Lipset has expanded and refined his use of these varied data, for example, carefully avoiding his earlier tendency to lump together Canadian francophones and anglophones. Lipset has also greatly expanded his use of nonquantitative data, particularly in a chapter entitled "Literature and Myths: Canadian Perspectives." Indeed, Lipset's mastery of the data is the strength of this book, which is extensively documented with 690 notes occupying 53 pages and a 34-page bibliography. Advanced scholars and beginning students alike will be the beneficiaries of Lipset's prodigious scholarship, and both will appreciate his engaging style. More than any other single scholar, Lipset has through his work validated his claim that "knowledge of Canada or the United States is the best way to gain insight into the other North American country" (p. xiii).

How will *Continental Divide* affect the theory and methodology of political science? Here the verdict is less positive. To his credit, Lipset explicitly juxtaposes his culturalist approach with other structuralist approaches. He pursues this theme in several chapters and devotes a section of his concluding chapter to it. However, since so much of Lipset's argument rests on the notion of "formative events," he should have undertaken a more thorough theoretical discussion of it. Lipset mentions the significance of events such as the Civil War (p. 197) and the Depression (p. 164) but does not relate them to his notion of formative events. Indeed, the discussion of Weber's loaded dice analogy itself is rather cursory.

Those unconvinced by Lipset's methodology will nevertheless learn a great deal from his immense scholarship. For many years to come, serious students of U.S. and Canadian politics will turn to this work as the leading comparative study of both countries. They will find particularly challenging Lipset's conclusion that the United States is currently suffering an even more severe identity crisis than Canada—a country obsessed with identity concerns since its inception and whose continued existence seems constantly in question.

DAVID V. J. BELL

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Understanding Peasant China: Case Studies in the Philosophy of Science. By Daniel Little. New Haven: Yale University Press, 1989. 322p. $30.00.

Students of agrarian structures and movements and rural development in China, Asia, and even elsewhere are fortunate that Daniel Little chose their bailiwick in which to exercise his skills as a philosopher of social science. Little, not a researcher in matters agrarian or Asian, has chosen these Western scholarly literatures as a launchpad for his examination of "the logic of social inquiry" (p. 1). He makes a persuasive case that philosophers of social science ought to move beyond their specifically philosophical and somewhat abstract approach and direct their attention to the trenches of actual scholarly work in order to promote both better social science and better philosophy.

Little focuses his attention on four lively substantive controversies and myriad methodological and epistemological issues. He begins with the "moral economy debate." The substantive issue here is whether (as James Scott argues) peasant society is structured and driven by reciprocal relationships and cultural values (among peasants or between them and landlords) oriented to shared concerns with subsistence, resulting in redistributive and risk-aversive behavior and practice or is really the more anomic world of interest maximization (as Popkin has proposed). Little focuses on Southeast Asia and, contrary to the book's title, does not raise the thorny issue of why these theories have proven difficult to apply to China. He uses the debate to expose and analyze rational and public choice theories, game theory, materialism and idealism, functionalism, and even positivism (implicitly in his comments on the differences between natural and social science).

Little also plays the role of the careful reviewer, closely analyzing the logical and evidentiary bases of the debate. The arguments are detailed and illuminating but also appropriately balanced: Little ends up unable to resolve the evidentiary contradictions that have puzzled many scholars, calling for more studies and complaining that in both Scott and Popkin's books "the data are thin" (p. 66). But he does offer a pointed critique of Scott for making an idealist argument that begins with culture; this itself will be controversial, since Scott can also be (and usually is) read as starting from a materialist premise about subsistence. He praises Popkin for his rational choice approach but chastises him for his "thin motivational assumptions" (p. 67). Little mobilizes a sophisticated understanding of rational choice and game theories that scholars of rural Asia would do well to emulate. A major project of Little's work is to try to forge a compromise between rational choice approaches and cultural relativism by incorporating culturally specific forms of intentionality into the calculus of the rational agents.

The second debate concerns the work of G. William Skinner on China's structures of rural marketing and macroeconomic regionalism. Here the theoretical issues are less pressing and primal, but for the China field they may prove more stunning. Little confines himself more to the role of the careful critic subjecting arguments to close evidentiary and logical scrutiny. He concludes that "not much [of Skinner's] quantitative reasoning survives. . . . All that survives is the qualitative reasoning" (p. 104). Skinner's work has been extremely influential in historical and more contemporary work on China by anthropologists, economists, geographers, historians, political scientists and sociologists. If Little is right, much of it now rests on the shakiest of evidentiary (though not theoretical) ground. One looks forward to the replies by Skinner and those sympathetic to his work.

The third debate concerns the reasons that China failed to make a breakthrough to economic modernization much earlier than it did. These are issues of high interest to comparativists working on economic development. Here Little's choices are curious. He focuses on a sharp critique of Kang Chao's demographic theory and pays appropriate attention to the exploitation-based explanations of Carl Riskin and Victor Lippit. But he ignores two major approaches, Mark Elvin's (emphasizing technology) and Etienne Balazs's (focusing on the state).

Finally, Little turns to the question of peasant movements. Here the larger issues of his chapter on the moral economy debate return, enriched further by his commentaries on local politics, political culture, and the differences between more narratively historical and theoretically self-conscious scholarship. Works represented emphasize millenarian (Naquin), neo-Marxist (Marks) and local politics (Perry) approaches. Sensibly, Little finds that each illuminates part of the problem but is insufficient in itself. He concludes with a plea for holism but offers only the briefest and vaguest sketches of how it might be applied.

The book returns from these forays into scholarship on rural Asia with three chapters on issues in the philosophy of social science: generalization, explanation, and empirical reasoning. They are too substantial and wide-ranging to summarize here but are recommended for their cogency, clarity, and sheer good sense. Their basic theme is a plea for compromise and synthesis among epistemological and methodological traditions.

In a sense, *Understanding Peasant China* is itself an example of what can be accomplished by synthetic work; for it is a most welcome new development in the growing eclecticism of social science. One can only hope that it will inspire some of Little's philosophical colleagues to make other spheres of social science the beneficiaries of their scrutiny. Or perhaps social scientists could turn the tables by bringing to bear on relevant areas of philosophy their insights about the world and their reflections on their own efforts to understand it.

MARC BLECHER

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Strong Societies and Weak States: State-Society Relations and State Capabilities in the Third World. By Joel S. Migdal. Princeton: Princeton University Press, 1989. 296p. $45.00 cloth, $10.95 paper.

State-Building in the Contemporary Third World. By Su-hoon Lee. Boulder: Westview, 1989. 191p. $30.00.

The nature, relative roles, and capabilities of the state in most contemporary Third World countries have generated intense scholarly debates and controversies. Although both Su-hoon Lee and Joel S. Migdal have attempted to account for the difficulties and variations in the ability of the Third World states to exercise social control and manage and process competing and conflicting demands from autonomous groups within these societies, the two scholars differ in their methodological approaches and theoretical conclusions.

Lee concentrates on how the international political economy creates opportunities and imposes constraints on the ability of the Third World state to perform extractive, coercive, and incorporative functions while Migdal discusses the precarious balance between the state and other societal forces, emphasizing the centrifugal factors that have undermined the capacity of the Third World state to maintain effective social control. Lee posits a dominant hegemonic role for the state in the Third World, but Migdal refutes the state-centric paradigm and focuses on the tenuous accommodation between state elites located in the capital city and entrenched localized strongmen in the countryside, who have devised successful strategies of survival and are engaged in competition with state officials in the provision of tangible, divisible benefits and in the control of effective protection and patronage—clientelistic networks.

For Migdal, the state in the Third World remains weak and is indeed subverted by the actions of the provincial-based strongmen and power brokers who maintain alternative organizational structures and are even more able to garner the loyalty, support, and obedience of the masses or rural population. In short, center–periphery relations in most Third World countries remain an endless struggle for social control between state elites on the one hand and local moneybags and petty despots. Migdal lucidly demonstrates how and why the local strongmen appear to be winning the battle for societal control and explicates this predicament by unraveling the complex historical, sociocultural, and politicoeconomic factors during the precolonial, colonial, and independence eras that have mitigated the emergence of state hegemony over the intensely heterogeneous and deeply fragmented societies of Third World countries. Indeed, Migdal's thesis seriously questions the conventional wisdom most predominant in the literature on state corporatism and the rise of bureaucratic authoritarianism in most Latin American countries.

On the other hand, Lee's discussion of state building in the contemporary Third World marks a crucial departure and even contradicts Migdal's thesis of the existence of weak states' structures that are challenged and subverted by strong societal centrifugal propensities. Utilizing a panel model of quantitative analysis of aggregate cross-national data, Lee argues that the state in the Third World has grown stronger than other societal groups and, in fact, maintains hegemonic social control in terms of the expansion of extractive, coercive, and incorporative organizational mechanisms.

According to Lee, state dominance over society in the contemporary Third World can be sufficiently explained by the positive effects of transnational linkages and the magnitude of each peripheral state's entanglement in the capitalist world economy. A Third World state's incorporation in the world political economy is positively correlated with its domestic expansion of coercion–extraction capabilities and mass educational integration function. However, state expansion of war machines and internal resource extraction efforts can stifle the growth and provision of higher educational facilities, thereby limiting the state's ability to perform the societal integration function.

Contrary to the dependency theories that emphasize the negative effects of Third World countries' engagement in the international political economy (i.e., underdevelopment, pericentricity, monocropism, and neocolonialism), Lee's study strongly suggests that transnational forces are crucial and even beneficial for the existence and consolidation of strong states in the Third World. Consequently, when the state in a Third World country is weak, it can be cogently explained by the low level of transnational incorporation and not necessarily because of existence of entrenched domestic nonstate actors and power centers competing for mass allegiance thus challenging state for social control, as postulated by Migdal.

In essence, the two books address the dilemma of state building in modern Third World countries. Despite their different methodological approaches, divergent focuses, and polarized conclusions, they represent important studies and even complement each other by enhancing our knowledge about the changing roles, dynamics, predicaments, and prospects of the state in modern Third World countries.

Lee's study concentrates exclusively on the impact of external factors such as international trade, transnational linkages and interstate wars and rivalries as independent variables that account for the existence and viability of strong states in modern Third World countries. Lee's model disregards whatever effects the presence of powerful domestic nonstate actors create and ignores the type of social class alliances or even how variations in societal group configurations can and do enhance or mitigate the ability of the state to expand its politicoeconomic organizational and social control mechanisms. In fact, no mention is made about how the political and sociocultural histories of the deeply divided and pluralistic societies of the state-nations in the Third World weaken or strengthen state capabilities.

Lee's ahistorical and qualitative-deductive-based study raises some questions: What exactly is a strong state? Which groups in these heterogeneous societies perceive the state as strong? How strong is the strong state over time and are all Third World states equally strong even if they are fully embedded and benefiting from the existing international political economy? How do we account for variations in state capabilities within each country or across nation-states? No doubt, we need to go beyond statistical quantifications, which are usually subject to controversial debates with regards to their efficiency and predictive effectiveness as tools of measurement of the strengths and relative weaknesses of the state's ability to exercise social control over existing endemic fissiparous pressures in most post-independence Third World societies.

Unlike Lee's panel-regressive-based analysis, Migdal's discussion is essentially a comparative historical synthesis of how internal and external political, socioeconomic factors account for the inherent weaknesses of most Third World states struggling to maintain social control in the postcolonial period of statehood and nation building. Migdal discusses state-society relations in terms of the probability of the existence of a weak or strong state by emphasizing the importance of whether institutional mechanisms and strategies exist to ensure group survival or not, whether they are centralized or decentralized, and whether they are controlled either by state elites or locally entrenched strong men. States are weak if state elites fail to control the provision distribution of economic resources and legitimating symbols or if provincial strongmen (business class, owners of large real estate, or local strongmen) manipulate, subvert, or utilize state bureaucratic structures to enhance their power base and thereby undermine state goals and aspirations at the implementation stage. Although the thesis is compelling and analytically insightful, the conclusion lends credence to the pessimistic prognosis that most modern Third World states are not engaged in state building but plagued by state disintegration. It is not clear that strong states in the Third World are predicated on the establishment of democratic regimes or based on the use of heavy doses of terror and instruments of coercion as manifest in most military dictatorships in Third World countries.

The case studies on Egypt, Israel, Mexico, and Sierra Leone are truly enlightening; and the judicious references to Nigeria, Ghana, South Africa, India, and other Third World countries further enhance the scope and level of theoretical generalizations. Also, the rigorous argumentation about the prevalence of weak states in the Third World and the preconditions for the existence of strong states (as the Israeli case study demonstrates) poses the question why the proliferation of subethnic separatist and irredentist movements have not overintensified in the numerous pluralistic and divisive state-societies of the contemporary Third World. In fact, societal pluralism is the exception rather than the rule throughout the Third World, and state control over society appears to be stronger than Migdal's analysis would suggest.

It is common knowledge that state elites in most Third World countries are confronted with challenging and difficult balancing acts of how to stimulate economic development, redress communal violence, regional disparities in the allocation of state controlled resources and, above all, how to maintain political stability, regime legitimacy, and foster national integration. Clearly, some states in the Third World have been more successful than others in establishing social control and effective organizational mechanisms to address these problems; but this phenomenon can hardly be sufficiently explained by the rational choice model that is predicated on survival strategies and organizational command networks. Migdal's analysis does not deeply explore the cultural normative basis of mass allegiance and loyalty to the state, which is crucial for the perpetuation of strong states. No doubt, the entrenched local elites also control culture goods and other forms of psychic gratifications, in addition to economic resources and protection infrastructures.

In sum, both Lee's *State-Building in the Contemporary Third World* and Migdal's *Strong Societies and Weak States* are well-written and impressive additions to the growing literature on state structures and the dynamic processes inherent in the postcolonial peripheral nation-states of the Third World. While Lee emphasizes the external factors facilitating the expansion and strengthening of state capabilities, Migdal deals with both the domestic and external causes for the prevalence of weak states that are challenged, undermined, or sometimes overwhelmed by intrasocietal centrifugal forces. The fact that the findings of both authors are different and even contradictory does reflect the diversity in analytical perspectives, research methodologies, and the possibilities of varied interpretations of the dynamics of state and nation building in the Third World.

PAUL OROGUN

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Intifada: The Palestinian Uprising. By Don Peretz. Boulder: Westview, 1990. 246p. $38.50 cloth, $14.95 paper.

For over a century, control of the former British mandate of Palestine has been a matter of dispute between Zionists and Palestinians. The Palestinian uprising against more than 20 years of Israeli occupation of the West Bank and the Gaza Strip began in December 1987. Since that time, it has became the most significant, widespread, and sustained expression of Palestinian nationalism since the Arab Revolt of 1936-39. Peretz's survey of the Intifada is a detailed description of the multifaceted situation that led to the uprising, the current policies of the Israeli government toward Palestinians, the impact of the Intifada on the political, social, and economic institutions of Jewish Israelis and of Palestinians, and the attitude of the international community during the first 22 months of the uprising. This well-written volume is one of the first single-author books on the Intifada; as such, it is a valuable contribution for any scholar interested in the evolution of Israeli-Palestinian relations. Peretz also provides the necessary context to make the Intifada more readily comprehensible to nonspecialists.

It is difficult to write on a continuing conflict—there is always the danger the book will be outdated even before it is published. *Intifada: The Palestinian Uprising* stands up surprisingly well despite the massive changes in the international community that have occurred since its completion in 1989. This is because the Intifada reflects a fundamental conflict between two national groups—an occupier and an occupied—over control of the land and its people. This basic dynamic has not been greatly affected by the transformation of Eastern Europe, the influx of Soviet Jews into the Occupied Territories, Israel's internal political difficulties, or the modest shifts in U.S. policy toward Israel.

Peretz is strongest when discussing Israeli governmental policies and the response of U.S. and Israeli Jews, particularly the Israeli Left, to the Intifada. He does a skillful job of weaving in illuminating quotations from individual citizens and government officials to give a sense of the variety of reactions; his summary of Israeli public opinion polls provides further evidence of this diversity. Peretz rightly concludes that at least initially, the impact of the Intifada on Jewish Israelis was far less profound than its impact on the Palestinian community: "In short, while the Intifada had its costs, they were relatively small for most Israelis—in terms of Jewish lives, far smaller than those in any of Israel's five or six wars. After nearly two years of confrontation with the inhabitants of the territories, barely a score of Israelis had lost their lives, fewer than in any month of traffic accidents and fewer than a twentieth of the Arab lives lost. The fact that few had suffered led to acceptance of the situation" (p. 157). This ability of the average Israeli to virtually ignore the Intifada is a crucial point that is often overlooked.

Peretz is less insightful when he is writing about Palestinians. This may be in part a result of his sources. Peretz relies heavily on the *Jerusalem Post International* (it accounts for fully half the notes), a paper that although until recently it reflected an editorial policy slightly to the left of the mainstream Israeli population, is not and could not be expected to be overly sympathetic toward, or understanding of, the Palestinian perspective. In light of Peretz's lengthy experience in the Middle East, it is surprising he did not make greater use of other Israeli sources, such as published translations of articles from the Hebrew press, or a wider range of Palestinian sources, such as the English language weekly newspaper *al-Fajr.*

Peretz seems reluctant to move beyond description to analysis and interpretation. In a laudable effort to explicate the complexity of the situation and the multiple interpretations given to specific events, he often resorts to presenting competing viewpoints without assessing the validity of the claims. For example, at one point Peretz summarizes the public accounts of two Israeli reservists who were part of the unit that occupied, and laid seige to, the West Bank village of Kabatiya for more than five weeks in early 1988. One soldier was highly critical of how the occupation was conducted and related a series of horror stories about callousness, insensitivity, and brutality. The second reservist accused the first of "hyperbole, exaggeration, distortion and pure invention" (p. 124) and presented his own very different version of the military's actions in Kabatiya. Peretz's only comment regarding these two interpretations is

> The range of attitudes among soldiers sent to quell the uprising reflected the differing public perceptions of the Intifada. Often soldiers serving in the same unit, carrying out identical tasks in a single Arab village, came away with diametrically opposing views of what they saw and of the necessity for their actions. . . . As these two contrasting accounts of events at Kabatiya demonstrate, the attitudes and perceptions of observers could differ depending on what they read in the press, saw on television, or heard on the radio (pp. 122, 125).

While Peretz's point is well taken, it ignores a number of critical issues imbedded in these two accounts and ultimately is not satisfying.

In a related concern, Peretz indicates one of his goals is "to determine why [the Intifada] is unique" (p. 4). This is a curious comment since there is virtually no subsequent discussion of this issue or any reference to the massive theoretical and empirical literature on domestic instability, ethnic conflict, revolutions, colonial wars of independence, and so forth. To be sure, the Intifada does have elements that set it apart from other expressions of political conflict; but it would have been valuable to have known the criteria on which Peretz made his claim. Despite these problems, however, Peretz's book shines a light on some important aspects of the Israeli-Palestinian relationship and is a welcome contribution.

DEBORAH J. GERNER

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Transition to Technocracy: The Structural Origins of the Soviet Administrative State. By Don K. Rowney. Ithaca: Cornell University Press, 1989. 236p. $31.95.

Rowney's discussion of the origins of the Russian-Soviet administrative state deals with the gradual emergence of technocratic elements and sets the stage for their dominance within a fully mature Stalinist system. Focusing both on the growing importance of such technocrats as a consequence of reforms of the czarist system during the second half of the nineteenth century and on the emergence of a new, presumably more reliable technical intelligentsia under the Soviet regime, this study makes an important contribution to our understanding of the emergence of the "new class" of Soviet administrators whose elevation to the status of an "official intelligentsia" both contributed to the rapid development of the Soviet Union as an industrial power and then hobbled its ability to respond creatively when the conventional wisdom of the Stalin era—and the efforts of cautious reformers who followed the *Vozhd*—proved to be poor guides to the future.

Rowney poses a key question that students of any revolution must answer: How much do revolutions change? More importantly, how are revolutions changed by a process of reality testing in which revolutionaries and opportunists alike bend rhetoric and form to their own purposes? The answer, as Rowney admits, is not simple, especially when measured in terms of the evolution of administrative organs in the hope that their composition and structure will reflect a trend toward greater reliance on technocrats while simultaneously embodying the social and political realities of the day.

If the most important contribution of Rowney's work is to enlarge an already-not-insubstantial body of findings that confirm that the presence—and, more debatably, the power—of technocrats grew as a consequence of both pre- and postrevolutionary reforms, it is also important to note that he has found further evidence that the reform process brought unintended yet still significant consequences. Most significant are the findings that elements of the society found ways to exploit both organizational changes and the official policy of fostering the emergence of a technocratic elite. In the prerevolutionary case, elements of the nobility found it possible to win positions within the growing bureaucratic order, thus preserving the substance, if not the mechanisms, of their ascendancy. More importantly, Rowney points out the mutually dependent relationship between what he terms a "would-be preelite" of prerevolutionary junior-level functionaires who aspired to upward mobility after 1917 and the Stalinist regime, which built and then consolidated its hold on power not only by purging the prerevolutionary technocrats but also by emerging as the champion of the new Soviet intelligentsia whose rise to power confirmed the regime's intention to create a qualified yet politically loyal administrative class.

Rowney's work is detailed and exacting—and sometimes overwhelming for the wealth of detail in light of his less ambitious attempts to weave fact and theory into a broader framework. It stands as an important contribution beside earlier works such as P. A. Zaionchkovsky's *State Apparatus of Imperial Russia in the Nineteenth Century* (1978), John A. Armstrong's *Soviet Bureaucratic Elite* (1959), Kendall E. Bailes's *Technology and Society under Lenin and Stalin* (1971), and Sheila Fitzpatrick's *Education and Social Mobility in the Soviet Union, 1921–1934* (1979).

Despite these considerable strengths, Rowney's study inevitably begs the question of origins versus behavior, that is, whether any amount of exacting knowledge of where bureaucrats come from or how they are trained will tell us the most important things—as opposed to just interesting things—that we need to know to understand their behavior. To be sure, all such studies face the same dilemma, and most resolve it in similar fashion—by taking the leap of faith that, as Rowney puts it, "the organization is mainly its people." But it is more: many different people pursuing conflicting goals; people working within different administrative units, each with its own mission and sense of identity; and, perhaps most importantly, people who learn and change, sometimes through gradual pressures from their peers and a process of bureaucratic socialization and sometimes through conscious exploitation of the agencies within which they work, as Rowney admits. To know who they are or to know what they have learned is to know much—but not all.

DONALD R. KELLEY

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Capitalism and Socialism in Cuba: A Study of Dependency, Development, and Underdevelopment. By Patricia Ruffin. New York: St. Martin's, 1990. 212p. $39.95.

The Stroessner Era: Authoritarian Rule in Paraguay. By Carlos R. Miranda. Boulder: Westview, 1990. 177p. $33.50.

These books present contrasting perspectives of the influence of history, culture, economics, and the international system on the processes of development and regime consolidation in Cuba and Paraguay.

Ruffin studies the development process in Cuba under both capitalism and socialism. In part 1 she explicates the structure of Cuba's dependent relationship on the United States and the capitalist world economy and how it has made the island vulnerable to the economic and ideological dictates of the United States, perpetuated developmental retardation and a succession of corrupt, authoritarian governments, and reinforced race and class divisions as defined by the export-oriented sugar monoculture. These economic and political inequalities advanced the island's nationalist and revolutionary potential.

Part 2 chronicles the constellation of social, economic, and political forces informing the transition from Cuba's unique brand of tropical socialism after 1959 to its subsequent embrace of the "Soviet road" in economic planning. In moving to a socialist command economy fully integrated within a Soviet-dominated sphere of influence, Ruffin asserts that "Cuba traded its dependence on the United States for a new and different dependence on the Soviet Union" (p. 136). Cuba's Soviet dependence does remain qualitatively different from that of the past in that the Soviets do not own the Cuban means of production; Cubans enjoy a greater relative autonomy from Russia than they did from the United States; and undeniable improvements in living standards occurred under socialism for the majority of Cubans. But the island's relationship with the USSR remains akin to the earlier dependence on the United States regarding trade, division of labor, indebtedness, and failure to diversify and industrialize.

Ruffin's "pox on both houses" analysis will surely palliate North American modernizationists wincing from two decades of *dependencista* anticapitalist and anti-U.S. remonstrance. Capitalist or socialist, dependency results from great power chauvinism and Ruffin's balanced interpretation reflects this reality. However, like any paradigm, dependency emphasis on exogenous, structural factors reflects a bias in understanding objective internal conditions also constraining development. Ruffin makes no mention of the dysfunctionality of Fidel Castro's overriding charismatic authority, disdain for organization, and penchant for personal intervention in retarding the institutionalization of bureaucratic organs—even those as basic as the Communist party—concomitant to rationalized modernization.

Uncritical acceptance of the basic premises of *dependencia*—which more comfortably (often justifiably) place blame for developmental retardation on exogenous rather than endogenous factors—conveniently steers Ruffin away from even cursory attention to other valid questions her study raises. In Cuba's "struggle for autonomous development" (p. 72), to what model does *la lucha* strive? Albania? Why has Castro remained slavishly doctrinaire to the imported Soviet Economic Reform Model that even now is repudiated in Eastern Europe and being transformed in the USSR? Why is "people's democracy" (p. 164) in a Stalinist power structure failing to extend political trust and branding all factional organization counterrevolutionary and demanding active (but only positive, conformist) participation? The absence of a broader, more critical perspective of Cuban developmental retardation remains the major weakness of what is otherwise a good explication of the specificity of Cuban dependence on the United States and Soviet Union.

Attention riveted to Cuba is matched only by the scholarly neglect and historical dearth of interest concerning Paraguay and the Stroessner regime. Carlos Miranda's excellent study, coming a decade after Paul Lewis's seminal *Paraguay under Stroessner* (1980), is a comprehensive and timely analysis of Latin America's "forgotten" nation and dictatorship.

Miranda's solid scholarship and clear writing style untangles the myriad influences of history, sociocultural values, and institutional arrangements that characterize Paraguay's heritage of authoritarian structures and beliefs. Elements crucial to creating an intellectual and political environment conducive to the steady ascendancy of authoritarian executives include an exaggerated tradition of caudillismo; clientelist and corporatist political arrangements; militarism; and (as in Cuba) political confusion, economic collapse, and foreign domination between 1870 and 1932.

Chapters 4–6 analyzing the machinery of Stroessner's dictatorship are of particular interest for students of authoritarian regimes. Miranda demolishes common, ill-informed generalizations dismissing Stroessner as a political neanderthal. Stroessner's durability rested on more than just ham-fisted repression. An unquestioned administrative capacity, coupled with a penchant for details, long work hours, and a ruthless obsession for personal power helped the dictator overcome initial opposition from within the Colorado party, bureaucracy, and military. Miranda correctly shows Stroessner's dictatorship to have been personalistic, not military. Stroessner was not simply primus inter pares in a contemporary junta; he was the strongman, totally dominating the entire regime, including the army and single-official party. While not charismatic, Stroessner secured a popular base to his regime by bringing the century-old Colorado party under his formal control; a key difference from other contemporary right-wing regimes in Latin America, such as Argentina (1966–72, 1976–82), Brazil (1964–84), and Chile (1973–89). Miranda shows mass acceptance of Stroessner stemming from the unique, personalist, authoritarian heritage of Paraguay. He also discusses Stroessner's manipulation of the myths and values of the nation and his control of the official Colorado party, as well as the penetration and politicization of the military, and the limited economic development and corruption that glued the regime's elites together in a felonious web of complicity.

Only chapter 4, concerning the "ideological bases" of the Stroessner era evokes semantic hairsplitting. Miranda subsequently hedges his bet, shifting to the term *doctrine.* The Stroessner regime in fact possessed no explicit, intellectually elaborate set of ideals. *Stronismo* never becaue a genuine comprehensive ideology. The regime's social and political ideals conformed to Juan Linz's concept of a vaguer "mentality." A simple conservatism, narrow patriotism bordering on mania, virulent anticommunism, blind allegiance to Stroessner, and a determination to maintain control informed the regime. No larger utopian or transformative vision existed.

The two volumes come to complementary conclusions regarding the impact of geography, politics, history, and culture on regime consolidation. Both Cuba and Paraguay produced militarized, repressive regimes dominated by personalistic caudillos who struggled to maintain national autonomy vis-à-vis powerful neighbors (the United States in Cuba's case, Argentina and Brazil in Paraguay's). Ruffin provides a useful, limited, interpretation of how foreign imperialism accelerated Cuba's switch to socialism and Soviet dependency as an alternative developmental model. Miranda details Paraguay's peculiar history and culture to present the definitive study to date concerning the rise and demise of a little-known but longstanding authoritarian regime. *The Stroessner Era* is a major contribution to Latin American area studies.

PAUL C. SONDROL

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Rethinking the Pacific. By Gerald Segal. New York: Oxford University Press, 1990. 400p. $64.00.

Following his earlier work on geostrategic relations among the major powers of Northeast Asia, the author attempts to determine whether the concept of a forthcoming "Pacific century" has any basis in fact. His thesis, that "there is no important cultural, ideological, political, economic, or even military sense in which it is particularly useful to talk to 'the Pacific' " (p. 377), contrasts with the fact that he takes four hundred pages to do just that. Is his exercise "useless" in his own terms? Clearly, the main problem is that the argument is analytically barren, with serious problems in concept and in measurement.

Conceptually, he compiles his list of Pacific countries by looking at a map rather than by aggregating the cultural subregions of North America, Pacific South America, the South Pacific, Southeast Asia, and Northeast Asia, an approach that would pose the question how one subregion relates to another in a larger Pacific context. When he discusses the Atlantic community, however, he refers exclusively to the North Atlantic. Had he included Atlantic Africa, the Caribbean, and Atlantic South America in the scope of the Atlantic community, he might instead have come to the conclusion that there is more subregional linkage in the Pacific than in the Atlantic. Instead, he repeatedly notes how the Pacific community falls short of the Atlantic community.

Empirically, no explicit measurements are proposed for determining when a world region can be "usefully" discussed; but Segal uses three implicit criteria to identify the coherence of a "community": cultural interaction density, existence of a security community, and economic integration. Other possible criteria, such as the scope and number of activities of regional institutions, are excluded despite the centrality of their role in the literature on international and regional community building.

Lacking a coherent scale of measurement, he presents data on the Pacific that clearly fall somewhere between extreme ends of the community–noncommunity continuum. His conclusion, that the region is a noncommunity, would have to be modified if he had taken the trouble to interview some of the foreign policy makers and participants in myriad cooperative efforts throughout the region.

Insofar as the author seeks to present an impressionist rather than a definitive account, a judgment on the heuristic character of the book must be based on the extent and quality of information presented, even though various readers might differ over generalizations from the data. In this regard the author indeed provides a lot of data; but once again, there are two flaws.

First, data sources vary from citations of scholarly works without specific page citations to daily and weekly periodicals without sources, so that the facts are somewhat frivolously documented. Second, while the author's expertise in regard to Northeast Asia comes through clearly, I counted 65 factual errors and eight ethnocentrically disparaging remarks concerning other regions, a listing too long for a mere erratum slip.

Before analyzing the success in attaining a Pacific community, it is appropriate to ask whether leaders and masses in Pacific subregions in fact want to become integrated components of a contemporary Greater East Asia Coprosperity Sphere. The eight pages on the *politics* of efforts at community building in the Pacific at the end of chapter 24 thus provide the author's finest contribution. It is a pity that they were not the centerpiece of his concerns.

MICHAEL HAAS

University of Hawaii

The Indochinese Refugee Dilemma. By Valerie O'Connor Sutter. Baton Rouge: Louisiana State University Press, 1990. 256p. $27.50.

The world refugee problem—persistent, tragic, and intractable—has spawned the new interdisciplinary field of refugee studies and with it a spate of discipline-specialized books. One recent political science addition to refugee literature is Valerie O'Connor Sutter's *The Indochinese Refugee Dilemma.*

Sutter approaches the Indochina refugee question from the perspective of national interests and their relationship to humanitarian concerns. The author attempts to consider five avenues of inquiry: (1) the foreign policy interests of the refugee-producing states (Vietnam, Laos, and Cambodia), the neighboring refugee-receiving states of first asylum, (Thailand, Malaysia, Singapore, Indonesia, the Philippines, and Hong Kong), and the nations whose regional interests affect superpower politics (China, the United States, and the Soviet Union); (2) the domestic social, economic, and political interests of these nations as they affect, and are affected by, refugees; (3) the comparative U.S. response, based on national interests, to the Hungarian and Indochinese refugee exoduses; (4) the role of the United Nations High Commissioner for Refugees (UNHCR), international law, refugee definitions, and obligations of states; and (5) the possibilities—resolution, stalemate, or expansion of the crisis.

Sutter begins her analysis with a case study of the Hungarian refugee crisis. The United States' response was affected by the presence of World War II refugees and escapees from the Soviet bloc countries, the Cold War, humanitarianism and foreign policy, and an awakening to the rights of refugees. There is little that is new or particularly innovative in her treatment of the Hungarian refugees. Because of the relatively small size of the Hungarian influx, short duration, limited resettlement costs, and its composition of skilled and/or educated white Europeans allied to a well-established resident ethnic pressure group of conationals, it is marginal as "the historical background against which we can view other refugee crises" (p. 49). For theoretical purposes, from a normative political perspective, a comparison with the United States' response to the European refugee crisis before and during World War II would have been more instructive.

Nonetheless, Sutter's point that "the Hungarian refugee problem foreshadowed characteristics that are intensified in the Indochina refugee issue . . . [and] showed how national interests influenced refugee policy and colluded and collided with humanitarian concerns" (p. 52) is well taken. She is on target with her observation that "the conflict between humanitarian values and national interests poses a dilemma for the Indochina refugee case, just as it did for the Hungarian refugee problem (p. 52).

Most refugee advocates reject an exclusively idealistic-moralistic posture and recognize the imperatives of vested national interests. But they would disagree with Sutter's beliefs that the United States has "generous annual [refugee] admissions" and that "refugee relief funding are ample testimony to American compassion and magnanimity" (p. 53). Admissions and funding are elastic concepts.

The chapters "National Interests and the Roots of the Exodus," "Thailand As a First Asylum Country," and "Other Asylum Countries" are accurate, well researched, and concise but comprehensive and illume the intricacies and nuances of the Indochinese refugee dilemma. The chapter "The United States and Resettlement" is a misnomer. The author discusses primarily admissions. Moreover, while foreign policy influences admissions, the author neglects other equally essential factors such as the magnitude of the numbers of admittees, the costs of resettlement, and domestic pressure groups.

The discussion of the UNHCR, under the rubric "Humanitarian Concerns," accurately proceeds "from the premise that this organization must operate not with the authority that characterizes sovereign nations but with limits imposed on it by the nations that created it and on which it must rely for funds" (p. 190).

The Indochinese Refugee Dilemma concludes with the observation that "the predicament in the Indochina refugee case exists in the tensions among competing national interests and between those interests and humanitarian concerns" (p. 216): "It is only in the narrow margins where interests and ethical considerations intersect, then, that refugees can be aided" (p. 221). And while Sutter concludes her investigation "without insight into a magic formula for resolving this most perplexing refugee dilemma" (p. 224), she advances a few previously suggested ameliorative ideas such as sharing out long-term refugee camp residuals, more flexible diplomatic postures, and development assistance.

The Indochinese Refugee Dilemma is a useful, well-researched, thought-provoking examination of a human tragedy.

NORMAN L. ZUCKER

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Out of the Ashes: Life, Death, and Transfiguration of Democracy in Chile, 1833–1988. By James R. Whelan. Washington: Regnery Gateway, 1989. 1,120p. $49.95.

This is a carefully researched political history of the vicissitudes of democracy in Chile by a prominent journalist, but it is marred by a doctrinaire conservative orientation. The author is familiar with a wide range of events, personalities, and sources relating to modern Chilean political history, reflecting his 14 years dedicated to the preparation of the book. Informed vignettes are presented of key political actors, political parties, and institutions such as the church and the various armed services at different historical junctures. Over one hundred personal interviews are listed, including several with each of the members of the military junta as well as with former presidents Frei and Allende.

The book is very long (1,120 pages) and complex, but it is never dull to read. The journalistic background of the author is always evident through the vivid, clear writing style and the quick and often exciting pace of the narrative. This an impressive accomplishment in a book occupying well over a thousand pages covering a host of topics.

While the political history begins in 1833, the first three chapters quickly cover the period up to the early 1960s in 129 pages. Chapters 4–7 cover the period from the early 1960s up to the 11 September 1973 military coup overthrowing Allende and take the narrative up to page 454. Chapter 8 deals with the coup itself in over a hundred pages. Chapters 9–12 and a final section, "Reflections," carry the chronicle nearly up to the end of the period of the military junta and amount to about half of the total text of this massive volume.

The large page allocation directly related to the military junta (chaps. 8–12 and the final section for a total of nearly six hundred pages) reflects the political admiration of the author for the conservative orientation and policies pursued by the junta from 1973 to the end of the 1980s. Whelan's thesis about the place of the military junta in Chilean history is disarmingly simple. The military junta, Whelan claims, did not destroy democracy in Chile, since the dynamics of the Chilean democracy did this itself in the period leading up to 1973. On the contrary, military rule during most of the 1970s and all of the 1980s reimposed order on chaos, thereby establishing a more solid foundation for subsequent political democracy and economic progress. Essential to the economic and political successes of the military junta were exclusion of radical political elements from national political life and disengagement of the state from the economy while pursuing a free market approach.

It is, of course, correct that the military junta was generally able to impose order and bequeath a growing economy to the elected Aylwin government in early 1990. Whelan's recurring complaint is likewise well founded that an international double standard militated against the junta whereby its accomplishments (economic and political) were denigrated or ignored while its shortcomings (especially human rights and oppression) were often exaggerated or held to be worse than other, more heinous international perpetrators. Whelan particularly faults academics (especially political scientists) for exercising this double standard.

It is well that Whelan provides a solidly documented defense of the military junta's policies, especially since criticism of these policies has often been so vitriolic as to exclude any recognition of positive qualities. What is worrisome in the case of Whelan is that his conservative preferences lead him to glide over the very real and negative features of the 1973–90 period of military rule. For Whelan, human rights abuses were unfortunate but exaggerated; and in any event the suppression of political competition was necessary, especially in light of past and continuing excesses of the Left. The institution of political stability plus the economic expansion experienced during the latter years of military rule suffice, for Whelan, to give the military junta high marks. In sum, according to Whelan, during the 1990s Chilean civilians may choose to build on the new forward-looking context inherited from the military or to return to misguided policies and chaos of the past.

While one can acknowledge that there were some positive dimensions to Chilean military rule, it is hard to endorse such a static black-and-white interpretation of the junta's policies of 1973–90. Let it merely be noted that those of varying political persuasions have argued convincingly that it's a period with which subsequent civilian governments will have to cope (see, e.g., Mark Falcoff's *Modern Chile, 1970–1989: A Critical History* [1989] and Alejandro Foxley's *Chile y su futuro: Un pais posible* [1987]). A balanced interpretation, while acknowledging some positive aspects of junta rule, would need to recognize as well that sustained and oppressive military dictatorship aggravated a series of difficult problems for subsequent civilian rulers. Whelan rightfully rejects all the leftist polemics coloring interpretations of modern Chilean political history; but his own assertive, right-wing interpretation may unfortunately generate still more polemics and detract from the real value of the book.

MICHAEL A. MORRIS

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INTERNATIONAL RELATIONS

Chemical Warfare, Chemical Disarmament. By Valerie Adams. Bloomington: Indiana University Press, 1990. 276p. $29.95.

Chemical Weaponry: A Continuing Challenge. By Edward M. Spiers. New York: St. Martin's, 1989. 218p. $45.00.

At a time when the United States faces the possibility of chemical warfare from Iraq and with the background of the continued proliferation of this "poor man's nuclear weapon," these two studies are particularly apropos. Spiers seeks "to examine the evolving nature of chemical warfare and to proffer some assessment of the developments in agents, munitions, protective kit, and operational tactics and . . . to speculate upon the feasibility of achieving . . . a Chemical Weapons Convention" (p. vii). His analysis constitutes an updating of his earlier work, *Chemical Warfare* (1986). Adams covers historical and contemporary material that he believes will answer the question "How did we get into our current position and where should we go from here?" (p. xi).

Although both books go over much similar ground, they complement each other rather than simply overlap. Furthermore, in several places, their coverage of different material allows them usefully to supplement each other. Only Adams, for example, devotes the better part of an entire chapter to an examination of the proposed text of the Chemical Weapons Convention currently before the UN General Assembly. She concludes that it "meets the main Western concerns . . . for a high degree of verification of stockpile destruction and nonproduction" while similarly satisfying "Soviet concerns in so far as it embraces the US binary programme and provides for a considerable degree of control over the chemical industries of the West" (p. 195).

Commenting from the perspective of a year earlier (August 1987), however, Spiers is considerably less sanguine. Even "negotiating a regional European agreement at a time when proliferation appears to be gathering pace in the Third World, and when Iraq is flagrantly flouting the Geneva Protocol, seems hardly apposite" (p. 140). He concludes that a Chemical Weapons Convention "would not necessarily curb the prospects of chemical warfare recurring" (p. 144).

Adams nurses the uninitiated reader through technical definitions better than Spiers, devoting most of her first chapter to this useful task. She also offers a table of main chemical warfare agents (p. 10), including their technical and common names, persistency (how long they remain effective), and types (irritant, incapacitant, or lethal).

Both authors agree that (as Spiers puts it) "chemical weapons are neither particularly effective in themselves nor potentially decisive on the field of battle" (p. 40). Rather, their "most important role . . . is probably that of acting as 'force multipliers' . . . by compounding the effects of other munitions, and by compelling an adversary to . . . adopt [deliberating] chemical avoidance procedures where possible" (pp. 40–41). In the jargon of chemical warfare, this would lead to "degradation," or the impairment of performance.

In addition, each author points out the great unpredictability of bacteriological (biological) warfare, which makes it a less suitable weapon than chemical warfare. They have their greatest fear, however, for the military application of genetic engineering or biochemical weapons. Spiers warns that "the transfer of certain genetic traits into naturally infectious microorganisms could potentially create organisms of greater virulence, antibiotic resistance, and environmental stability" (p. 153).

Both studies cover the history of chemical warfare and its uses, verified and reputed, since World War I. Adams declares that U.S. activities in the Vietnam War "saw the largest scale use of chemicals in combat of any modern conflict" (p. 73) and "let the CW genie out of the bottle" (p. 84).

She also states that "in 1985 the Iranians were close to achieving a breakthrough when the Iraqis launched their chemical attacks" (p. 89). Spiers adds that Iraq "encouraged the proliferation of such weaponry . . . by demonstrating that a nation can develop and deploy these weapons in defiance of international censure and with scant regard for international law" (p. 125).

However, U.S. accusations that the Soviets used chemical warfare in Southeast Asia after 1975 foundered on the ridiculous, observes Spiers, when the reputed victims "could not distinguish bee feces from the purported spots of a chemical warfare agent" (p. 120). Both authors agree, though, that (as Adams has it) "chemicals of some kind have been used in Afghanistan" by the Soviets (p. 103).

Each study examines the Soviet chemical weapons capacity—Adams skeptically noting, however, that "to some extent, a review of the Soviet capability . . . entails a review of the source material, rather than the subject matter" (p. 120). She argues that the Soviet-American chemical-bacteriological warfare programs can be understood "against a complex background of threat assessment, institutional pressures, available technologies, military doctrine and, for the US at least, financial considerations" (p. 166).

In analyzing Soviet chemical warfare doctrine, Adams possibly gives too much relevancy to Marshall Sokolovskii's now dated comments first published in 1962 (pp. 128–30). Certainly, Gorbachev's initiatives will have to be considered in the future more than either of these studies were able to.

In closing, Adams notes that "the *possession* of chemical weapons is legitimate in international law; their *use* is prohibited by the 1925 Geneva Protocol" (p. 207). Nevertheless both authors detail the repeated uses of chemical warfare over the years, and Spiers ominously warns that "compared with atomic devices, chemical weapons are . . . easier to design and produce, involving a relatively low level of technology" (p. 139). Accordingly, Adams argues, "in the longer term, a comprehensive and global convention backed by a rigorous defensive programme seems likely to offer greater assurance of security" (p. 222).

Both studies are well documented and contain excellent bibliographies along with adequate indexes. Factual and well written, each analysis will be a useful addition to the literature.

MICHAEL M. GUNTER

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The GLOBUS Model: Computer Simulation of Worldwide Political and Economic Developments. Edited by Stuart A. Bremer. Boulder: Westview, 1987. 940p. $139.00.

Disarmament and Development: A Design for the Future? By Stuart A. Bremer and Barry B. Hughes. Englewood Cliffs: Prentice Hall, 1990. 240p. $20.67. paper.

In the middle 1970s, at the Institute for Comparative Social Research of the Science Center Berlin, a team led by Stuart Bremer began to develop a global computer model based on a research program developed by Karl Deutsch. Their goal was to create a computer nation-based political world model from empirical data. After a considerable effort, the GLOBUS model was in working order by 1985. This model is considerably more complex than other large-scale models used to analyze aspects of global change. The simulated world of GLOBUS has 25 states, each state consisting of six sectors (national economy, political system, demographics, government budget, foreign economic relations, and foreign policy), along with worldwide subsystems for international trade and international politics. Thus, unlike most of the other global models, GLOBUS is oriented toward political and economic factors. It has approximately 40 thousand variables and parameters, and versions run on both large mainframe computers and microcomputers, simulating the time period 1970–2010.

The scale of this effort naturally leads to the question whether it was a worthwhile project. This question has two main components. First, is the model a sensible one, that is, does it accurately reflect our knowledge about nation-states (or our best guesses if no firm knowledge exists)? Second, what use can be made of the model? Two recent books shed important light on these matters. One is edited by Bremer and describes the GLOBUS model. The second is a book authored by Bremer and Hughes that uses GLOBUS to study the interactions between conflict and development.

The edited book is a massive description of the model. Separate chapters are devoted to each major component of the model, as well as chapters or appendixes introducing the project, evaluating it, and describing the simulation and its mathematical aspects.

The heart of the book is the set of chapters on the components. Each begins with a review of the literature, and the theoretical orientation adopted by GLOBUS. This is followed by a detailed review of the model components, as well as a discussion of the parameters and initial values. Finally, illustrative results and future developments of each component are discussed. Aside from providing a good description of the GLOBUS model and the reasons for the choices made by the team, each chapter gives a clear picture of the appropriate empirical literature and the degree to which it forms a unified, coherent whole. Hand in hand with these descriptions, one develops a sense of the history of the GLOBUS project and how choices were made.

On the whole, one can conclude that the choices made by the GLOBUS team were good ones, although not necessarily the same as others might have made. As well, while one can admire the initial decision to build on empirical findings, one develops the sense that the literature often did not provide a firm enough base to build a simulation. This forced the GLOBUS team to conduct its own analysis and/or make design choices without significant empirical evidence. Although unavoidable, this had an impact on the final form of the model. Finally, since the construction of the model involved "linking islands [of theory] separated by oceans" (p. 4), one can ask whether the existing literature can really be pulled together to form a large-scale working model of global political and economic elements.

In sum, the edited book provides a wealth of detail not just on the GLOBUS model itself but also on the justification for the specific elements of the model. Further, the book provides a series of very valuable reviews of significant portions of the international relations literature. The team is to be credited for producing a book that allows an outsider to make an informed judgment about GLOBUS.

Given the massive effort that was put into making GLOBUS a reality, what exactly can be done with it? In *Disarmament and Develop-*

ment, Stuart Bremer and Barry Hughes provide an answer:

> The GLOBUS model is [to be] viewed not as a 'crystal ball' capable of forecasting the future but as a laboratory in which experiments about the future can be carried out. . . . The results of a model run are not seen as constituting a prediction of what inevitably is to be but as the outcome of a very complex experiment that, we hope, tells us something about just how much control we have over the future." (pp. 17-18)

Their complex experiment deals with the impact of a dramatic reduction in arms expenditures by the East and the West, on development in the South.

GLOBUS is very suitable for this type of inquiry. First, the linkages between military spending in the developed world and economic development in the developing world are complicated; and it may be difficult to represent this complexity using another type of model. Further, since we have not witnessed the scale of arms reduction postulated by Bremer and Hughes (a 33% reduction over five years), it is difficult to address this question relying on empirical data.

Bremer and Hughes consider several types of relationships in their study: (1) the relationship between arms reduction, northern economic performance, and East-West relations; (2) the relationship between arms reduction and increased northern aid to the South, (3) the relationship between increased northern aid and improved southern economic performance, and (4) the relationship between southern economic performance, southern political stability, and North-South relations. Thus, the Bremer and Hughes model does not postulate a direct impact of arms reduction on development but rather a chain of effects that leads from arms reduction to southern development.

The results of GLOBUS indicate that a drastic change in East-West armaments would produce only a small change in southern economic performance. Further, this small change is only possible if the decrease in armaments is tied to a large increase in northern foreign aid, along with changes in southern fiscal policy and southern currency valuation.

Naturally, one's assessment of the validity of these findings is dependent on one's assessment of the utility of GLOBUS. But even if one has a disagreement with the underlying premises of the GLOBUS model (for example, Bremer states that the domestic economic process component of GLOBUS "rests on a solid classical foundation" (p. 45), the results of the analysis are thought-provoking. They suggest that southern economic performance is not as malleable as many analysts have assumed and further that the links between southern economic performance, southern political stability, and North-South relations are not as simple and direct as many have concluded. Thus, these GLOBUS results should serve to stimulate further empirical and theoretical work on these linkages.

To conclude, the GLOBUS team has provided us with an intriguing global model integrating a wide variety of factors and a large number of actors. The two books reviewed here collectively provide a complete description of the model and its foundations in the theoretical and empirical literature and demonstrate how GLOBUS can be used to address interesting questions involving complex global linkages.

RICHARD J. STOLL

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Uncertainty and Control: Future Soviet and American Strategy. By Stephen J. Cimbala. New York: St. Martin's Press, 1990. 183p. $39.95.

The tension between a nuclear strategy designed to deter war and a strategy that would be optimal if deterrence were to fail has been recognized since Glenn Snyder's *Deterrence and Defense* (1961). Deterrence can be achieved by persuading an adversary that it would face costs disproportionate to any conceivable gains in a war—the more certain and terrible the costs, the greater the deterrent value. If deterrence were to fail, the existence of options to end the conflict with minimal destruction would be imperative.

Stephen Cimbala argues that the United States and the Soviet Union have developed a relationship that deters the initiation of major war while neglecting policies needed to control such a war if deterrence fails. He offers three hypotheses to explain this state of affairs: (1) U.S. and Soviet military doctrines are "heavily biased toward the unrestrained use of force once coercion has turned into violence"; (2) the historical experience of both "has conditioned their military leaderships toward skepticism with regard to any limitation of aims in war"; and (3) neither side has command systems "adequate for the control of major coalition war under present or near-term conditions, *even if* the fighting remains below the nuclear threshold" (pp. 1–2).

Cimbala's fundamental contention, that major war between the superpowers would be uncontrollable, is well supported. He leads the reader through a discussion of the theoretical problems associated with the relationship between uncertainty and stability, follows with an examination of technical problems associated with command and control, and continues with a juxtaposition of U.S. and Soviet strategies for nuclear and conventional war. The interaction of the two countries with their different strategies and limited command-and-control systems would create a cycle of misperception and a high probability of escalation.

Less well supported is the argument, implicit throughout the book, that an improved ability to control major war is necessary. Cimbala claims that "escapes from previous nuclear crises have been more by dint of luck than management" (p. 37). He later dismisses the argument that fear of nuclear escalation contributes to the deterrence of conventional war with an assertion that the argument assumes that the decision to use nuclear weapons "is a cool and deliberate one made by rational actors" (p. 81). But rationality is a matter of degree. Individuals in an "irrational" state of anger are more likely to punch drywall than glass windows. Leaders are more likely to lash out with nuclear weapons if their use appears to be controllable than if the result is seen as Armageddon. The crucial question, which Cimbala never fully confronts, is whether the advantages associated with improved control if deterrence fails are outweighed by the increased probability that deterrence will fail if war seems less cataclysmic.

One profound problem confronted by Cimbala and others writing on national security is presented by the fluidity of events. Contemplation of a book is an act of courage at a time when weekly news magazines can become dated before publication. Nevertheless, recent changes in the international system have not been kind to Cimbala. His extended discussion of a NATO–Pact conflict has been largely superceded by events. The danger of escalation appears more remote as the Soviets adopt a more defensive strategy. Cimbala makes an effort to keep up with events. There are references to "new thinking" and "Soviet defensivism." But the new information sometimes appears added rather than integrated into the analysis. For example, a section that begins with an argument that the Warsaw Pact is more likely than NATO to remain cohesive during a war concludes with an admission that "recent ferment in Eastern Europe makes a war-stable pact coalition even more remote" (pp. 142–45). But we must still assume pact stability in a conflict because the Soviets will simply avoid war in its absence. The conclusion not only calls into question the general thrust of the section, it contradicts a major theme of the book by suggesting that the Soviets have nearly perfect control over the decision to go to war.

Most unfortunately, despite its subtitle, the book has little to say about the future. Cimbala explains why escalation control is unlikely, but does not suggest how it could be achieved. There are no prescriptions for changes in command and control, no proposals for a more controllable strategy. The conclusion is brief and does little to integrate preceding chapters into a complete argument. It appears almost as though the author grew tired of the struggle to keep up with events and is saving discussion of future strategy for more stable times.

VAUGHN ALTEMUS

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Saudi–Yemeni Relations: Domestic Structures and Foreign Influence. By F. Gregory Gause III. New York: Columbia University Press, 1990. 233p. $35.00.

The relationship between the states of the Arabian peninsula has long been the most neglected part of the study of Middle Eastern international relations. In addition to reflecting the dominance of the Arab–Israeli and Soviet–U.S. conflicts in most literature on the region, this neglect is also a result of the opaque character of much foreign policy making and implementation in that area. Yet for all the difficulties involved and the impact of these other rivalries, the international relations of the Arabian peninsula have a separate, distinct character that has evolved almost unnoticed by most observers, whether in the Middle East or elsewhere.

Among the issues influencing the international relations of the peninsula three have particular prominence: (1) the relationship between the six oil-producing monarchies and the two (until recently) poorer republics of Yemen; (2) the interaction of the specific local identities (Omani, Saudi, Yemeni, etc.) and the broader Arab context; and (3) the impact on the inter-Arab and broader international scenes of social and political upheaval within these states. If to these are added some of the overarching rivalries of the Middle East (Arab-Israeli, monarchical-nationalist, Arab-Iranian, secular-religious), it becomes evident why, beneath the appearance of regional marginalisation, the peninsula has been an area of such interstate tension over the past three decades.

It is the great achievement of Gregory Gause's study that he charts one of the most contentious aspects of interpeninsula relations and does so with authority and clarity. His work covers Saudi relations with both North and South Yemen up to the early 1980s, showing why Saudi Arabia was so concerned about the two Yemens, what the instruments of its influence were, and how success in fostering a client government in North Yemen contrasted with failure to prevent the consolidation of a radical, pro-Soviet, regime in the south. Gause explains this differential outcome by reference to two factors: (1) the limits of the Saudis' power and decisiveness, which led them to miss opportunities for adequately challenging the regime in Aden and (2) the relatively more cohesive character of the South Yemeni state, which was such as to lessen the impact of external pressures.

The implications of the Saudi-Yemeni case for the comparative study of international relations are well brought out in Gause's work and his explanation of different outcomes is plausible. There are, of course, other ways of telling the same story. The cohesiveness of state and society is itself a product of the different ideological characters of the two Yemeni states: either could be derived from the other. There are also two areas that, relying as he does largely on public sources, he somewhat neglects: (1) the depth of Yemeni popular hostility to Saudi Arabia, a sentiment born of territorial conflict and the experience of Yemenis as migrant workers in Saudi Arabia, and (2) the fragmented character of Saudi decision making on foreign policy issues, something evident in other contexts.

Two events since the completion of Gause's work provide illuminating confirmation of its main themes. One is the proclamation of unity between the two Yemeni states in May 1990, which was in part motivated by common concerns in both San'a and Aden about Saudi influence and was designed to yield a stronger state in Yemen as a whole. The other is the support given by the new Yemen Republic to Iraq in the August 1990 conflict over Kuwait: faced with continued anxiety about Saudi influence, the Yemeni rulers sought to reinforce their countervailing alliance with Baghdad. The parameters of Gause's study have thus more than retained their relevance.

FRED HALLIDAY

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Autonomy, Sovereignty, and Self-Determination: The Accommodation of Conflicting Rights. By Hurst Hannum. Philadelphia: University of Pennsylvania Press, 1990. 503p. $44.95.

This valuable volume chronicles the issues that arise from disputes over minority rights when the state is seen to represent majority groups better. The book is divided into three parts. The first introduces the book's mission—"to examine the ways in which international law and domestic constitutional arrangements can contribute to resolving disputes between minority and majority groups" (p. 3)—and then devotes a chapter to elucidating each of several key concepts in international law that help to define the problem.

The most thought-provoking is chapter 3's discussion of the tension between the principle of sovereignty and the right of peoples to self-determination. Hannum documents that when framed in the context of decolonization, the right to self-determination has been upheld at the expense of the claimed sovereignty of a colonial master but that when the issue has concerned the desire of a minority for secession, the sovereignty of the state has been regarded as the superior claim.

Chapter 4 traces the history of international protection of minorities from the Treaty of Westphalia and the Congress of Vienna, noting its limited practical effect. For the most part, minority rights have been defined largely in terms of religion, language, and cultural activities. Moreover, Hannum perceptively observes both the sociopolitical realities that constrain international efforts (the hypersensitivity of states about their sovereignty) and the uncomfortable fit between the concept of "minorities" and the theoretical paradigms of the state rooted in social contract theory, Marxist class theory, and realist understandings of the community of states. Chapter 5 takes up the special case of indigenous peoples, while chapter 6 focuses on the newer doctrines of human rights, which concern the treatment by the state of individuals as opposed to groups. Flourishing under the United Nations after World War II, these "individual" rights have often been translated into "group" rights.

Readers interested in domestic conflict centered on minority issues will be well rewarded by the discussion of Part 1. The identification of key principles and themes is well conceived, and their discussion is elegant and thorough. The exhaustive notes provide a solid basis for further investigation of legal principles and institutional arrangements. This section will be a useful resource for those interested in international law but also a very accessible primer for those more oriented to domestic conflict who are seeking to augment their understandings with the legal dimension.

Part 2 is comprised of nine different case studies of situations that have yielded persistent, often violent, conflict over minority rights. Though they were chosen for their breadth and seeming diversity, Hannum argues persuasively that "there is an underlying similarity in potential resolutions that may be instructive" (p. 123). For each, a well-written narrative provides a compact history of the conflict, with an emphasis on the dynamics of political disputes over legal arrangements. Unfortunately, there is little new in these analyses and surprisingly little return to the issues described in part 1. The brief "Observations" sections of each chapter are disappointing. They most often pick up the theme of the need for greater flexibility in the political system, more courageous and farseeing political leadership, and greater tolerance for diversity but say little about how to bring about these desirable outcomes. Moreover, the analysis is weakened by the failure to draw into the discussion the more analytic writings on domestic conflict to be found in the literatures of political science and sociology. Still, researchers in these very areas have the most to gain from this volume. By drawing together similarly structured treatments of several diverse conflicts, Hannum has provided a valuable resource for those who are seeking patterns in the dynamics. The legal dimension contained in these chapters would be an especially welcome addition to analyses that more typically avoid such specificity.

Part 3 is designed to suggest the variety of political arrangements that have been utilized as a means to protect minority rights. While various types of federal structures are the best known of these, the documentation of a range of alternatives offers ample evidence that creative political leadership has a number of options for system design that have proved workable. In several of these instances, the international community has played a decisive role.

In sum, this volume is must reading for scholars interested in the legal dimension of minority conflicts with the state. It should also be required reading for the leadership of nations currently confronting such issues or the international actors who could be helpful in resolving them.

BRUCE E. MOON

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When Trust Breaks Down: Alliance Norms and World Politics. By Charles W. Kegley, Jr. and Gregory A. Raymond. Columbia: University of South Carolina Press, 1990. 337p. $32.95.

The basic argument of *When Trust Breaks Down* is that international norms matter and matter a lot. Kegley and Raymond claim that international stability is contingent on a normative order in which promissory obligations are binding. Conversely, "when commitments become brittle and allies waver, trust breaks down and chaos overwhelms community" (p. 263).

The authors "measure" the international normative order through a content analysis of texts written by international law experts ("publicists"), which, they argue, reflect the prevailing view on the degree to which alliance commitments are and should be binding. Taking the Congress of Vienna as their starting point, Kegley and Raymond delineate periods in which the norm that "pacts made in good faith are binding" (p. 88) held sway and others in which the norm that pacts may be unilaterally terminated ("if a fundamental change occurs in the circumstances that existed at the signing of the agreement" [p. 90]) predominated. The resulting cultures are characterized as restrictive and permissive, respectively (p. 49). Identifying which of these two cultures is most conducive to stability in the international system and determining the sources of norms transformation are the authors' central tasks.

Kegley and Raymond draw on long-cycle theory, hypothesizing that norms transformation coincides with global war and finding that nineteenth-century "system-transforming" war led to "norms which supported flexible conceptions of commitment," while twentieth-century global wars "have given rise to norms that supported binding commitments" (p. 145). A second set of hypotheses, derived from hegemonic stability-leadership theory, suggests that a restrictive culture will prevail in periods of hegemony while permissiveness will characterize a system in which power is more diffuse (p. 159). Finding empirical support for these arguments, Kegley and Raymond conclude that a restrictive culture is likely to hold sway when "military capabilities are highly concentrated and the level of alliance aggregation is high" (p. 175).

As for the consequences of norm transformation, the authors find that international crises are more likely to be managed peacefully in a restrictive culture (p. 216) and that the magnitude and severity of war among the major powers are greater in a more permissive culture (pp. 230–31). Peace, they conclude, "is best preserved when there is a moderate amount of flexibility in alliance initiations and partner choices, when commitments are made judiciously and not left open-ended, and when promissory obligations are considered binding by prevailing international norms" (p. 232).

This book's strengths include balanced and illuminating discussions of important theoretical, legal, and methodological issues entailed in the analysis of norms and a clearly written, well-organized presentation. Of note, too, is the sophisticated analytical approach combining case studies, contingency tables, and time series regressions.

Nonetheless, the treatment of a number of issues is inadequate or troublesome. For one thing, a more explicit description of their content analysis procedures (perhaps in an appendix) would have been helpful. More importantly, the authors do not devote sufficient attention to the problem of interpreting changes in alliance norms—changes that are measured on a scale from −1.00 (flexible) to 1.00 (binding). For example, the trend toward binding commitments characterized both the 1840–80 period (up .8 units) and the 1920–46 period (up .9) (p. 126). Yet the authors claim that nineteenth-century wars led to greater flexibility in alliance commitments while twentieth-century wars led to a norm of binding commitments (p. 250). Moreover, the authors appear to contradict themselves, since, in an earlier discussion, they argue that the Crimean War resulted in a more restrictive culture (p. 134). The problem here stems from the authors' focus on global wars when it is clear that a rapid transformation in alliance norms occurred, for example, during the period of European unification (1848–70).

Another issue the authors neglect is whether it is the trend in, or the absolute level of, commitments that matters. For example, the commitment norm underwent an identical decline (−.20 units) in the 1878–1914 and 1946–78 periods. However, since the latter decline began from a higher level (.40) than the earlier one (.00), it seems fair to ask whether the decline in these two eras is equivalent when the absolute commitment levels are so different.

Finally, there is some confusion over the generalizability of the findings to nonalliance agreements. Kegley and Raymond state early on that the "applicability of conclusions to all other types of agreements in general cannot be safely assumed" (p. 45); but they later apply their arguments to Cold War arms control negotiations (pp. 256, 274), which are not alliance, but interalliance, concerns. The authors do not explain whether the stability-enhancing "culture of trust" depends on the belief that commitments to allies are binding or that international agreements of all sorts should be kept.

Kegley and Raymond's work is a serious attempt to bring hard data and rigorous empirical analysis to the study of international norms, culture, and morality. As such, it makes a valuable contribution to an often soft and fuzzy field of international relations scholarship.

JANICE E. THOMSON

University of Washington

From Apology to Utopia: The Structure of International Legal Argument. By Marti Koskenniemi. Helsinki: Finnish Lawyers', 1989. 550p. $64.00 paper.

From Apology to Utopia examines international law from the perspective of the critical legal studies (CLS) movement. A disciple of David Kennedy, the Harvard Law School "crit" who first brought CLS to international law, Koskenniemi attacks "liberal political theory" and "the Rule of Law" as he strives to show that the law's cherished aspirations to objectivity and a distinctive legal methodology are illusions. For those acquainted with CLS, the villains of this book will be familiar; the argumentative techniques (of semiotics and deconstruction) will be predictable.

Law, according to this view, is indeterminate; on any given issue there are good arguments on both sides. "For the judge," says Koskenniemi, "it must ultimately remain an incomprehensible dilemma and source of professional frustration and personal disappointment why it is that equally competent lawyers, with equal amounts of professional integrity constantly come up with conflicting solutions to the same problems" (p. 493). The reason for the indeterminacy is that values come in opposing pairs: normativity contrasts with concreteness, idealism with realism, naturalism with positivism, deduction with induction, and "descending" argument with "ascending" argument. The *utopia* of this book's title is grouped with normativity, naturalism, and descending argument. *Apology* is grouped with concreteness, positivism, and ascending argument.

The author's choice of terminology is somewhat peculiar. At one point he defines *normative* as "being capable of impartial ascertainment and application" (p. 5). Scholarship is "utopian" when, "by lacking connexion with practice [it is] unable to demonstrate its norms in a tangible fashion" (p. 5). But why, then, is utopianism grouped with normativity? This makes little sense in terms of either his own definitions or the way that we customarily use those words. In addition, it is not clear whether these oppositions are, in fact, mutually exclusive as Koskenniemi claims. Why, for example, is normativity the opposite of concreteness? A norm might be highly abstract, or it might be relatively concrete. For instance, compare a general duty of charity to a particular individual's obligation to donate blood twice a year to the New York chapter of the American Red Cross. Similar points could be made about others of the author's "oppositions."

Setting aside such terminological questions, the author's point would seem to be that if we attempt to ground international law on state practice, our norms merely reflect what states currently do. This is what he means when he calls international law "apologist." But if our norms are unabashedly prior to, and independent of, state practice, they appear to lack normative foundation and are therefore unacceptably "utopian." International law is thus caught between a need to reflect what states in fact do and a desire to offer a basis for criticism. Koskenniemi notes that this duality is "hardly a novelty" (p. 5). Readers will probably recognize it as a variation of the argument that it is difficult to derive an *ought* from an *is.*

In one important sense, this book is not about international law specifically and the distinctive normative problems it poses. International law does have distinctive normative problems, because the absence of a central source of law or legal enforcement leaves nations in an apparent state of anarchy. However, Koskenniemi's arguments do not depend on this feature of international law but on failings of legal rules (such as indeterminism) that are present also in domestic cases. This is why he is able to borrow from domestic CLS reasoning wholesale and apply it virtually without alteration to the international setting. The examples he offers are international; his reasoning is not.

His destructive thesis complete, Koskenniemi turns in the concluding pages of the book to his alternative vision of what international law should be like. He claims that we must value "authentic commitment" (p. 488), face up to the fact that we must make value choices (p. 479), strive for broader legal imagination (p. 501), accept the reality of conflict (p. 486), and be realistic about the fact that international law cannot provide absolute certainty (p. 479). The legitimacy of a solution, he says, lies in the openness of the conversation leading up to its adoption (p. 487). He takes a strong stand against "imperialism" and "totalitarianism" (p. 497) and in favor of "integrity" (p. 494). Even those who are not convinced about the value of deconstruction will probably agree.

LEA BRILMAYER

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Vatican Policy on the Palestinian-Israeli Conflict: The Struggle for the Holy Land. By Andrej Kreutz. Westport, CT: Greenwood, 1990. 196p. $39.95.

Until a few years ago, the role of the Vatican in world affairs—particularly in the Middle East—was largely neglected by U.S. scholars. While there have been obvious limits to the ability of the Holy See to exert substantive influence with regard to conflicts in the Middle East and elsewhere, the Vatican on occasion has used its unique status and moral authority to influence thinking and actions within the Christian (especially Catholic) world—such as when it won strong backing for UN General Assembly resolutions calling for the full internationalization of Jerusalem. Even Arab, Palestinian, Israeli, and U.S. Jewish leaders have deemed it useful to make serious efforts to seek Vatican support for their respective views and positions. Thus, the scholarly study by Andrej Kreutz is both needed and welcomed because it adds to a better understanding of a largely overlooked but important subject.

After providing an informative account of the Vatican's institutions and foreign policy making process, the author discusses the historical evolution of Vatican interests, policies, and actions relating to the Holy Land from the closing days of Ottoman rule to the present. He shows how prior to World War II the Vatican was conservative and cautious; considered itself as primarily a Western institution; and backed Western colonial rule over non-Christian peoples in the belief that it had a civilizing mission to perform and could help contain the spread of communism and protect Christian (especially Catholic) interests in the non-Western world. Thus, it was happy to see British rule over the Holy Land after World War I and concerned when Britain gave up the mandate. It opposed the establishment of any non-Christian state (whether Jewish or Arab) in Palestine for fear that its interests and long-standing rights and privileges in the holy places, as well as the position of the Christian minority, would suffer from such a development. When the UN partitioned Palestine, the Holy See sought to salvage the situation by giving strong support for full internationalization of Jerusalem as provided for in the UN partition plan.

The author stresses that following the 1948 and 1967 wars, the establishment of an expanded Israel, and the emergence of major Palestinian refugee problems (many of the refugees were Christians), the Vatican sought to use its religious and moral authority and influence to help promote political as well as humanitarian goals, such as calling for refugees' right to repatriation; the need for international guarantees to protect not only the holy places and ready access to them but also the civil and religious rights and interests of the non-Jewish communities in Israeli-controlled territory; and a just and lasting peace settlement based on the right of both Israelis and Palestinians to self-determination and security. Vatican leaders also became more liberal in outlook and more concerned about the human, social, and national political rights of the peoples in the Third World. Moreover, with a rapidly increasing percentage of the world's Catholics now living in the Third World, the influence of the churches and peoples in that area has been growing within the Vatican and on its international policies and outlook.

The author provides a scholarly, objective, and copiously documented study of a highly sensitive and misunderstood topic. The main drawback is its brevity in dealing with some important topics, such as the Vatican and the Jerusalem issue; developments following the

1973 war resulting in a more moderate PLO position that, in turn, enabled Western Europe, the UN, and even the Vatican to support Palestinian rights; and the role of the Vatican during World War II.

Nevertheless, this is a valuable book that helps complement other worthy studies such as George E. Irani's *Papacy and the Middle East* (1986) and Kail E. Ellis's edited *Vatican, Islam, and the Middle East* (1987).

FRED J. KHOURI

Villanova University

Aid and Development. By Anne O. Krueger, Constantine Michalopoulos, and Vernon W. Ruttan. Baltimore: Johns Hopkins University Press, 1989. 386p. $45.00.

Can foreign aid promote economic development? Has it done so in the past? These are important questions, given the billions of dollars in foreign aid provided poor countries since the end of World War II. This book is a welcome addition to the literature on foreign aid which attempts to take a broad look at the nature and impact of that aid on recipient countries' economies. In contrast to much of the writing on foreign aid of the 1960s and 1970s and even today, this book, along with a handful of others (including Roger Riddell's *Foreign Aid Reconsidered* [1987] and Robert Cassen's *Does Aid Work?* [1986]), is mainly an analytical work rather than an advocacy piece. The goal of the book is to review the effects of development assistance on countries receiving it. The message of the book, repeated innumerable times, is that foreign aid has made a contribution to development in the past and can do so in the future but only where the economic policy environment is supportive of development and economic distortions are minimal.

The strengths of the book are in the scope of its analysis. Its exposition of the evolution of development thinking and of how aid has been thought to contribute to development is clear and succinct. Its reviews of the impact of aid on various sectors—infrastructure, agricultural production, and the quality of rural life, population, and health—is useful in pulling together evaluations and analyses from a variety of sources. Its country case studies of the impact of development (India, Turkey, Korea, and Ivory Coast, and Ghana), although not entirely consistent in their treatment of individual countries, provide a helpful measure of comparison among countries. The book, in short, brings together material on foreign assistance from a wide variety of sources and analyzes it in a clear and balanced manner.

The limitations of the book derive largely from its focus. It is relatively narrowly focused on the economic impact of development aid and does not, therefore, analyze the social or political dimensions of that aid. It is in these important areas where development attention is increasingly turning, based on the realization that politics and governance, as well as social structures and values in developing countries, play a major role in the effectiveness of development policies, especially structural adjustment policies. Moreover, its focus on development is a narrow one, emphasizing the economics of growth and putting rather less emphasis on the distributive aspects of development, which were so important in the 1970s and remain of considerable importance today in much of the academic community and among practitioners of development. The impact of Basic Human Needs policies, which shaped foreign aid projects for much of the 1970s, is given relatively short shrift with poverty alleviation as a direct focus of aid viewed rather as a deviation from the mainstream economics of development. It is true enough that evaluations of many Basic Human Needs projects by the World Bank and USAID have shown their impact to be disappointing: many collapsed after donor funding was terminated. But their impact on income distribution and their consequences in the medium term rather than the short term have yet to be explored in depth. A related point here is that the book excludes aid from nongovernmental organizations. This is an understandable exclusion, given the vast number of nongovernmental organizations involved in aiding poor countries and the dearth of evaluations of their projects. Yet the proportion of aid provided by these organizations is increasing, and they are becoming an important part of the development landscape. Their exclusion from this and other reviews of aid effectiveness may distort our understanding of the impact of foreign aid.

A further limitation of this treatment of aid is that it makes no attempt to distinguish among developing regions of the world in its assessment of the impact of aid. Why has aid been so disappointing in its impact in Africa, compared to its impact in Korea? If the answer is the "policy framework," why has the policy framework been so poor in Africa and changes associated with structural adjustment so slow to be implemented and so slow to have a positive impact on economic growth? These are critical questions both for academics interested in understanding aid and development and for practitioners in improving the effectiveness of aid. But they take us out of the realm of economics and so are little considered in *Aid and Development.*

In sum, the strengths of this book are that it brings together a wealth of material and analysis in one place and makes a strong (if repetitive) case for the importance of macroeconomic policies and microeconomic incentives to support efficient resource allocation if development is to occur. Its weakness is that it interprets "development" quite narrowly, confines itself to the economics of aid and development, does not break any new ground on development thinking, and is therefore limited in helping us confront the important and interesting development problems of today, many of which are found outside the discipline of economics.

CAROL LANCASTER

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U.S. Export Control Policy: Executive Autonomy Versus Congressional Reform. By William J. Long. New York: Columbia University Press, 1989. 172p. $30.00.

Commerce and Coalitions: How Trade Affects Domestic Political Alignments. By Ronald Rogowski. Princeton: Princeton University Press, 1989. 208p. $24.50.

These two books together bring to mind the adage about more being less. Ronald Rogowski seeks to explain political divisions within countries all over the world over the last 150 years. Long examines the evolution of one relationship—between the legislative and executive branches of the U.S. government—in one issue area over 40 years. While Long examines the effect of political concerns on trade, Rogowski attempts to build a framework for predicting the political alignments that will develop in society as a result of changing trade flows.

Rogowski was obviously smitten with the elegance of the Stolper–Samuelson theorem; but using it to analyze political alignments around the world over more than a century simply stretches its explanatory power—limited to start with—too far. In a 1941 article, Wolfgang Stopler and Paul Samuelson showed that factors of production with which a country is relatively well endowed will gain from trade liberalization (and lose from protectionist policies), relative to "scarce" factors. Applying the theorem to a simple political model with three factors of production—land, labor, and capital—Rogowski hypothesizes that as trade expands, the relatively abundant factors should become increasingly assertive and free-trading while the scarce ones become increasingly defensive and protectionist. When trade is contracting, the opposite should be true.

Students of trade politics will not be surprised to learn that import-competing sectors typically resist liberal trade policies while export sectors typically favor them. The trick is in identifying the sectors and predicting their behavior in a given situation. Rogowski all-too-cavalierly rejects work that raises doubt about the empirical validity of Stolper–Samuelson and ignores, or is not aware of, more recent trade research that further weakens the applicability of Rogowski's model to current events. The basis for the Stolper–Samuelson theorem is the Heckscher–Ohlin theory of trade, which says that countries will export goods that use intensively the factors with which they are relatively well endowed and import goods that use scarce factors intensively. Although widely used as a starting point for explaining trade flows, it is also recognized that there is a great deal of trade that does not fit in that model. For example, the growth in *intraindustry* trade among developed countries cannot be adequately explained by Heckscher–Ohlin.

Pre–World War II patterns fit fairly well into Rogowski's framework. Even there, however, Rogowski makes no attempt to assess the importance of trade relative to other factors in creating political cleavages; and (as he concedes) the model cannot explain why certain coalitions win over others politically, even when their economic power is declining. The fit of the model also seems to decline as Rogowski moves to the present. Trade negotiators in Geneva last fall would no doubt have been surprised to learn of "agriculture's demise" in Europe and Japan (p. 99). The assertion that since the war, most of Europe and Japan have embraced more open and liberal economic policies than the United States might also raise some eyebrows.

Long's task is far less ambitious than Rogowski's and in the end more convincing. He seeks to explain why Congress had had only minor success in its attempts to influence the executive branch on export control policy. His hypothesis is that "the executive has operated as an important and relatively autonomous political actor" in export control policy (p. 7). In addition to the greater latitude typically accorded the president by Congress on national security issues, the bureaucracy plays a more important role in this issue area than in others because of the complexity of the issues involved.

Long argues further that once the executive has been granted autonomy on a particular issue, it jealously guards its authority and independence even when circumstances change. Thus, the executive has successfully beaten back repeated attempts by Congress to reform the export control process, "despite rising business discontent (stemming from the vastly increased importance of exports to the U.S. economy), the end of the Cold War consensus, and a resurgence in congressional influence in foreign policy generally" (p. 105).

Long provides a brief but thorough review of the history of export control policy that provides substantial evidence to support his hypothesis. However, there is an aspect of the issue Long largely ignores that could further buttress his argument. Long's characterization has Congress opposing the executive on export control policy and trying to increase the weight given to commercial concerns. However, Congress added provisions requiring the imposition of controls on certain exports to countries suspected of supporting international terrorism. The previous year, Congress had passed the Nuclear Non-Proliferation Act controlling the export and reexport of nuclear materials and technologies. Proposed amendments to the EAA this year would further reduce coverage of export controls related to East-West trade but would expand them in areas primarily related to North-South trade and weapons proliferation concerns, especially products and technologies related to the production of chemical and biological weapons and ballistic missiles.

However, just as with East-West national security controls, the administration was able largely to implement the legislation in accord with its own interpretation of policy needs, rather than following congressional intent. President Bush vetoed the 1990 Export Administration Amendments Act, in part because of the provisions requiring the imposition of sanctions against countries using chemical or biological weapons or countries supplying such materials, arguing that Congress was trying to "micromanage" export control policy. Similarly, in implementing the NNPA and Fenwick amendment, the administration took full advantage of the loopholes provided for presidential discretion. These examples are too important to ignore; moreover, they support Long's thesis of executive autonomy.

In sum, Rogowski uses a wide-angle lens to capture a sweeping view of trade and political patterns over the past 150 years while Long uses a telephoto lens to focus in on the interaction between the legislative and executive branches of the U.S. government over export control policy. Rogowski's book is intellectually stimulating and interesting as history, but it is far less convincing than Long's as an explanation of how trade and politics interact and affect one another.

KIMBERLY A. ELLIOTT

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"A Winter of Discontent": The Nuclear Freeze and American Politics. By David S. Meyer. New York: Praeger, 1990. 295p. $47.95 cloth, $16.95 paper.

In the first term of the Reagan administration the Nuclear Weapons Freeze Campaign became one of the largest grassroots movements in the history of this country. In the months preceding the 1984 elections the challenge for the movement was to move from protest to policy. The freeze movement seemingly failed to meet this challenge; and almost as quickly as it arose, it faded from the political landscape. But the freeze cannot be judged as simply a success or failure. The story is more complex. David Meyer has captured that complexity in this study.

A bush-league activist in the movement itself, Meyer retreats to a rigidly-adhered-to scholarly stance. He sets himself three goals in the work: (1) to chronicle the "meteoric life" of the movement, (2) to develop a theoretical framework for understanding other protest movements in the United States, and (3) to integrate analysis of peace movements into an understanding of the policy context in which they emerge. To accomplish these tasks, he draws with discernment from social science literature and establishes a firm theoretical underpinning for his analysis of social movements and public policy processes. He then applies this framework critically to a detailed analysis of the politics of the freeze. The result is a readable, insightful case study of how the U.S. political system responds to such a challenge.

Meyer posits that the initial approach of the Reagan administration to national security policy created "the political space for a dissident movement" (p. 8). Traditional peace activists took advantage of that opportunity and broadened the appeal to the U.S. populace, which was deeply frightened by the specter of nuclear war and the resultant nuclear winter and consequently assumed a citizen's responsibility for "the fate of the earth" (p. 131). In the response, the freeze movement posed challenges not only to the content of U.S. national security policy but also to the policy-making process itself. The magnitude of the response quickly pushed the movement into the midst of the existing mainstream political institutions—interest groups, Congress, mass media, and political parties. It is Meyer's contention that these institutions channeled the force of this groundswell movement and domesticated it into seeking more moderate ends, thereby leaving the policy-making process untouched. In his view the freeze invested a great deal of energy and political capital in the 1984 election and in doing so precipitated its own demise. After that election, the movement splintered. Meyer holds that this came about because "mainstream politics fragmented the movement, defusing its political impact through "marginalization, depoliticization, and cooptation" (p. 254).

Nonetheless, Meyer concludes that the nuclear freeze movement did change the U.S. political landscape dramatically. On the surface it clearly failed in reaching its announced goals, but from the broader perspective it did play a major part in rescuing the arms control process that yielded, in short order, several Reagan–Gorbachev summits and the INF treaty signed in 1988. The movement was instrumental in the conversion of a popular president from an advocate of limited nuclear war to one committed to peace and the end of the Cold War. Thereby, Meyer concludes that the nuclear freeze movement serves as "a demonstration of a responsive democratic polity" (p. 272).

After the spate of advocacy literature for the freeze in the early 1980s, there have been several works that examine the freeze: Paul Loeb looked at the grassroot impact in *Hope in Hard Times* (1987); Pam Solo drew on her leadership experience in the movement for *From Protest to Policy* (1988); and Doug Waller contributed *Congress and the Nuclear Freeze* (1987). Meyer builds on these works and provides the first comprehensive examination of the nuclear freeze movement and its place in U.S. national security policy development, as well as in U.S. politics more generally. It will stand as the definitive study of the movement in the years ahead, as well as a valuable case study for the broader study of U.S. policy-making processes.

JAMES A. RHODES

Luther College

America's Secret Eyes in Space: The U.S. Keyhole Spy Satellite Program. By Jeffrey T. Richelson. New York: Harper & Row, 1990. 375p. $24.95.

Jeffrey Richelson has written an informative, interesting, and very readable book about the United States' spy satellite program. He has provided a helpful glossary, a chronology of events, an appendix on program summaries, and an extensive list of sources. The style of the book is descriptive and chronological. It is an excellent source for the reader interested in a general introduction to the arcane world of satellite intelligence.

Richelson's study provides considerable insight into the bureaucratic maneuvering among agencies and organizations with uses for satellite intelligence, from the battlefield interests of the military to the raw intelligence requirements of the CIA. While Richelson's book does not explicitly add to the discussion of how decisions are made in the national security establishment, his descriptive story adds considerable ammunition to the bureaucratic analysis of such policy.

Richelson provides many character sketches of the personalities responsible for developing the spy satellite program. These sketches are sometimes distracting, occasionally cluttering the narrative. At the same time, they provide valuable insight into the bureaucratic factors that affected the development of the program.

He also provides considerable insight into the argument concerning national security requirements for extensive secrecy versus the democratic society's need to have open discussions as the basis for public decisions. With persuasive evidence, Richelson contends that the U.S. satellite program would have been and will be more effective if a more open policy about satellite intelligence is maintained. For example, he suggests that "informed public debate concerning important public policy issues . . . suffers from the present secrecy. Some of those who advocate declassification of KEYHOLE [satellite] photography have suggested that it would allow the public to make a more informed judgment about aspects of U.S. defense policy" (p. 266). He provides considerable evidence in his discussion supporting that contention.

In reading about the use of KH-11 satellites for increased intelligence for battlefield surveillance and crisis monitoring, I could not help but wonder how the KH-11 figured into U.S. decision making prior to the Iraqi invasion of Kuwait. It is Richelson's contention that extensive satellite intelligence will be needed in the post-Cold War world: new sources of conflict will likely provide new crises. It seems apparent that the United States was surprised by the suddenness of the Iraqi attack on Kuwait. One can only wonder whether the United States had enough intelligence satellites to watch the Iraqi battlefield movements effectively prior to Baghdad's invasion or was watching something else considered more important at the time. Richelson's contention about the continued need for an effective spy satellite program certainly seems confirmed by the recent events in the Middle East.

One further argument, once again only implicit in Richelson's work, is that the best intelligence technology is still dependent on the priorities and value judgments of those deciding what intelligence information is important and ultimately interpreting that information to reveal an adversary's intentions. Richelson's analysis provides considerable evidence for this contention and will prove valuable to those interested in satellite intelligence.

ROBERT A. HOOVER

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Why Peace Breaks Out: Great Power Rapprochement in Historical Perspective. By Stephen R. Rock. Chapel Hill: University of North Carolina Press, 1989. 220p. $29.99.

A theorist bold enough to test hypotheses by making predictions about future events is a rarity. By analyzing cases of reconciliation between former enemies, Stephen Rock hopes to identify the factors that might lead to an end to the Cold War between the United States and the Soviet Union. Safe in the knowledge of hindsight, we can now test these theoretical predictions against current events.

He argues that friendly relations between enemies emerge in two stages. In the first, states decide that armed conflict is not an acceptable mechanism for settling their differences. In the second, leaders settle outstanding conflicts of interest. Rock's objective is to identify factors that cause states to decide that force is not an appropriate method for resolving their disagreements, in other words, why—rather than how—former enemies become friends. He does not analyze the *process* of rapprochement between former enemies (symbolic gestures, signals, public statements, minor agreements, and the like).

Instead, he weighs the impact of geopolitical conflicts of interest, economic interdependence, and ideological solidarity on states' motivation to settle their differences. Using a confusing and cumbersome terminology, Rock hypothesizes that a state of peace is most likely to emerge among states that are heterogeneous in the exercise of power, heterogeneous in their economic activities, and homogeneous in societal attributes. Because of the inertia in societal images, Rock posits that a crisis may be necessary to bring these favorable background conditions to policy makers' awareness by heightening the shared interest in avoiding war.

Rock tests these hypotheses by contrasting (1) the origins of the Anglo-French Entente Cordiale of 1904 and the rapprochement between Britain and the United States of 1895–1905 with (2) the deterioration of relations between England and Germany and Germany and the United States before World War I. For example, the geopolitical rivalry between Britain and the United States as naval powers was offset by their complementary trade and strong social, cultural, ideological, and language ties. By way of contrast, Germany challenged Britain's position as the leading naval power, and German manufacturers competed with British goods on world markets. Their growing military and commercial rivalry was exacerbated by the ideological cleavage between democratic, industrial Britain and authoritarian, agrarian Germany. While the literature on conflict resolution is substantial, this is the first comparative historical analysis of détentes. The case studies are succinct and enlightening.

What makes his cases suggestive rather than conclusive evidence is that he essentially presents a checklist of the presence or absence of the three conditions rather than integrating these variables within a more comprehensive theoretical explanation. Since none of these factors varies much over time, they can explain only why some states, as opposed to others, achieve a rapprochement, not why two particular former enemies become reconciled. Nor can this method of comparison easily identify spurious correlation. Further, it becomes hard to demonstrate causality. For example, the Fashoda crisis preceded the Anglo-French Entente by six years; without examining the motivations and calculations of the participants, it is difficult to cite the confrontation as the catalyst for the two powers' settling their differences.

By analyzing only long-term structural factors, Rock underestimates the role of contingency in historical events. In an interdependent, uncertain world, the process of interaction, including the participants' bargaining and diplomatic skills, helps determine the final outcome. The alignment of states before World War I might have looked very different had not the Germans been so greedy and maladroit in their diplomacy. Britain achieved a rapprochement with the United States from 1895 to 1905 through patient, unilateral concessions bordering on appeasement.

Another limitation on generalizability arises from the design of the study. To achieve comparability, Rock examines only cases from the pre-World War I era, which means that he cannot ascertain the effects of nuclear weapons on states' attitudes toward the use of force. This is unfortunate, because the specter of nuclear war arguably led the United States and the Soviet Union to conclude that force was not an acceptable means of resolving their differences as early as the Cuban Missile Crisis in 1962.

Was Rock right? The Soviet economic disaster has led to a retrenchment of Soviet geopolitical objectives, from attempts to play a global role to efforts to salvage a more modest position as a European power, reducing the conflict between U.S. and Soviet geopolitical interests. Further, the Soviets' renunciation of Marxism-Leninism and acceptance of pluralism has made U.S.-Soviet rapprochement more acceptable to conservatives within the United States. On the other hand, what made Mikhail Gorbachev's desire for peace credible was the Soviets' withdrawal from Eastern Europe, an area the United States long ago ceded to Soviet domination. Further, it was Soviet president Gorbachev, often against substantial domestic opposition, who initiated the series of events that led to the moderation in Soviet objectives. Rock provides a good analysis of the environmental incentives and constraints that predispose states toward enmity and friendship. To explain whether leaders act upon these possibilities, other factors need to be considered, such as the actors' diplomatic strategy, symbolic gestures, rhetoric, and costly concessions.

DEBORAH WELCH LARSON

University of California, Los Angeles

Book Reviews: International Relations

Allies in Crisis: Meeting Global Challenges to Western Security. By Elizabeth D. Sherwood. New Haven: Yale University Press, 1990. 245p. $25.00.

The Western Alliance after INF: Redefining U.S. Policy toward Europe and the Soviet Union. By Michael R. Lucas. Boulder: Lynne Rienner, 1990. 266p. $32.00.

One of the dilemmas that NATO has faced since its creation has been that of defining its goals in a changing world. That challenge has become especially critical recently, first with the onset of détente between the United States and the Soviet Union, then the subsequent revolutions in Eastern Europe and, most recently, the reunification of Germany. These events have profoundly altered the international system and have also changed dramatically the framework for NATO. With the diminishment of the Soviet threat and the transformation of adversaries into allies, the question of alliance goals—and the very future of NATO—becomes especially germane.

Threats confronting alliance members still exist. But those threats now tend to reside outside the geographic boundaries of the North Atlantic area. While this is not a new factor, what remains unclear is whether the alliance will respond to those increasing threats, whether it will seek to define its role in a changing world, and how it might do either. The works by Sherwood and Lucas attempt to address these important questions. Both books, however, face the problem of trying to answer those questions while the international system remains in flux. These volumes offer a framework within which it is possible to formulate questions as an important step toward ultimately finding answers.

Buried near the end of Sherwood's book is a statement that succinctly and eloquently sums up the out-of-area problem for NATO. She writes, "Only certain members had the will and the capability to act beyond the European theater" (p. 153). That has always been, and remains, the essence of the issue for the alliance members as they have attempted to confront events external to the boundaries of the North Atlantic Treaty area. It is the combination of the two factors—will and capability—that has influenced the response to any threat outside the treaty area.

Sherwood's approach is chronological, focusing on a number of international crises since 1948 that she feels are representative of the out-of-area problem. In so doing, she raises a number of important points regarding the triangular relationship between the United States, the European allies, and the external countries involved, many of which were former colonies. The implication here is that the European allies have often hesitated to get involved in such crises for fear of subjecting their own policies to intense scrutiny, something that most have been unwilling to do. Similarly, the United States often chose to pursue policies that minimized its own involvement with the former colonies of its allies.

Implicit in this analysis, however, is another way to explain the schism between the United States and the European allies. As Sherwood develops her case studies it becomes clear that the global dominance of the United States has come at the expense of allies—Britain and France. This, in turn, suggests an inevitable competition and confrontation between the three countries, which was manifested ultimately in the French withdrawal from the NATO unified military command structure and, ironically, the strengthening of the Anglo-American alliance.

Her analysis of the history of the out-of-area problem strikes at the heart of NATO policy when Sherwood questions what would happen if the allies were pushed to fight outside the European theater. The conclusion is that the European allies would claim that participating in such a conflict would have to come at the expense of the defense commitments at home, thereby shifting the burden onto the shoulders of the United States and concomitantly exacerbating the rift from one side of the Atlantic to the other.

The primary flaw in *Allies in Crisis* is the uncertainty about whether Sherwood's cases really represent the out-of-area problem or are simply examples of the complexity of current international politics. Even if an event affects one or more of the NATO nations it is not necessarily a NATO issue or an example of NATO's ability or inability to deal with that issue. Sherwood's case studies are interesting and well chosen, but at times she pushes the inferences too far in order to make the connections explicit. Nonetheless, the book offers a cohesive picture of what has been one of the consistent problems confronting NATO.

The Western Alliance after INF takes a very different approach to the problems confronting NATO today. Lucas begins by stating a basic paradox in current international politics: "East–West forms of cooperation that only a few years ago would have been considered a breach and weakening of military security are now finding acceptance as a higher form of security" (p. 1). He then uses this model of a shifting East–West security relationship to address the broader issues of East–West relations and NATO policy in general in the post-INF environment. Before proceeding, he does offer the important caveat that the foundations of defense policy are still shifting. Therefore, it is important to note that the emerging conception of security is one that must deal with a number of diverse factors—political, economic, military—simultaneously. While hardly innovative, this reminder is one well worth heeding.

Lucas divides his research into three parts, each dealing with a category of security interest broadly defined beginning with an overview of the political and military events surrounding the December 1979 INF decision, which he sees as critical in helping to redefine the subsequent security structure. He then addresses economic and technological issues as they have influenced, and will continue to influence, security issues, and concludes with an assessment and analysis of the new détente.

As part of his analysis Lucas offers three possible models or alternative visions to describe the future of NATO and the terms for a continuing European–U.S. relationship. While not exhaustive, these models represent an articulation of the complexity of the issues and suggest the difficulties inherent in trying to understand or define the future possibilities while the process is still underway.

Each of these volumes contributes to an understanding of the myriad issues facing the alliance today and raises dramatically the uncertainties of the future.

JOYCE P. KAUFMAN

Whittier College

China Eyes Japan. By Allen S. Whiting. Berkeley: University of California Press, 1989. 228p. $29.95 cloth.

Allen Whiting has written an authoritative study of one of the most important bilateral relationships in Asia. The current level of suspicion of Japan in China and the potential for region-wide conflict underscores the importance of understanding the dynamics of Sino-Japanese relations. Whiting's analysis of this vital relationship focuses on a crucial element affecting the development of relations—the role of images and misperception in aggravating and escalating tension. Building on his experience in government and on his earlier scholarship on the importance of images in Sino-Soviet relations and on China's entry into the Korean War, Whiting applies these concepts to China's contemporary Japan policy and to a discussion of the prospects for Sino-Japanese relations into the 1990s and beyond.

Whiting focuses on the 1982–1987 period, when a number of seemingly petty issues emerged as significant irritants in an otherwise mutually beneficial relationship. He explains that China's experience with the Japanese occupation from 1932 until the end of World War II shapes Beijing's interpretation of contemporary Japanese objectives such that Chinese leaders believe that revived Japanese "militarism" is a distinct possibility. In this context, Chinese leaders perceive each Japanese infringement on Chinese interests as a reflection of an underlying trend of an anti-China policy and of "militarism," which may ultimately surface as the dominant tendency in Japanese foreign policy. Thus, issues such as the content of little-used Japanese high school textbooks, unofficial visits by Japanese leaders to the shrine of war heroes, and a court case over ownership of a student dormitory are raised to the highest level in China so that Deng Xiaoping has personally raised such minor issues with Japanese statesmen. Moreover, China's harsh reaction tends to create an action–reaction cycle. Its vitriolic responses to ostensibly petty issues elicit Japanese perceptions of anti-Japanese sentiments in China and a rigid and escalatory Japanese response.

Whiting's analysis is not limited to these lesser issues. He applies this same framework to an analysis of Sino-Japanese economic relations and to China's perception of Japanese defense policy. Concerning economic issues, he finds that despite the relative apolitical nature of economic relations and the extensive level of mutually beneficial cooperation, Chinese images of Japan aggravate objective factors giving rise to economic conflict. Thus, while trade imbalances and the reluctance of Japanese businesses to invest in China reflect economic realities, Chinese leaders are apt to see a manipulative policy designed to maximize Japanese profits and minimize Japan's contribution to Chinese modernization. Thus, economic relations, which contribute the most to bilateral stability and conflict reduction, are also riddled with the baggage of history and play a more limited role in promoting consolidated relations than might otherwise be the case.

On the other hand, Japanese defense policy, which is the most politically sensitive aspect of relations and is the issue most pertinent to the recent history of Sino-Japanese relations, is primarily a source of tension. In 1986, when Whiting conducted his interviews in China, Chinese leaders appreciated the contribution of increased Japanese defense spending to containing Soviet power. Nonetheless, even at this time they viewed the Nakasone defense policy as a preliminary step toward the potential revival of Japanese militarism. Today, now that the Soviet threat has diminished, suspicion is even more prevalent in China, as revealed in Beijing's reaction to the suggestion that Japan might send unarmed soldiers to the Persian Gulf. Defense-related issues will have the most impact on future relations and current Chinese apprehension suggests that the action–reaction cycle that Whiting found over less central issues could come to characterize conflict over security issues.

The author's conclusions are sobering. Given the high level of mistrust in China and the prospect of ongoing bilateral tension and increased political and strategic Sino-Japanese competition in Asia, the sources of tension will likely expand. Under such a circumstance, it will require careful diplomacy, tolerance, and political stability in both Beijing and Tokyo to avoid heightened conflict. Yet, as Whiting's interviews reveal, with rare exception the level of knowledge in China regarding Japan is abysmal and the Chinese leadership, in an effort to warn Japan from pursuing its alleged "militaristic" tendencies, actually fosters greater society-wide anti-Japanese hostility. Indeed, much of the anti-Japanese sentiment among Chinese students reflects the impact of Beijing's Japan policy. Thus, as Whiting concludes, the challenge of maintaining stable Sino-Japanese relations will weigh heavier in Beijing than in Tokyo.

Whiting's conclusions are based on analysis of various Chinese publications and on his interviews in nine Chinese cities with government officials, foreign policy specialists, economists, students, and the editors of numerous Chinese periodicals. He has probed beneath China's immediate reaction to developments in Japanese politics and foreign policy to determine the underlying image shaping Chinese perceptions of Japanese policy. He has written an important work for understanding both Sino-Japanese relations and the future security of Asia.

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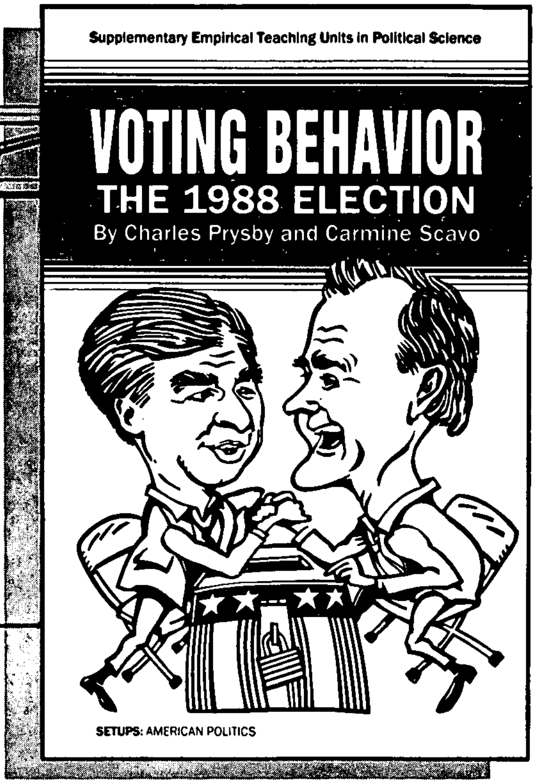

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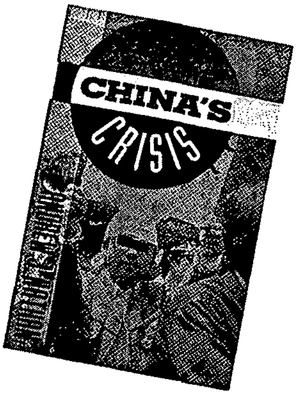

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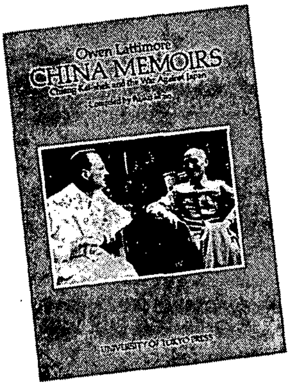

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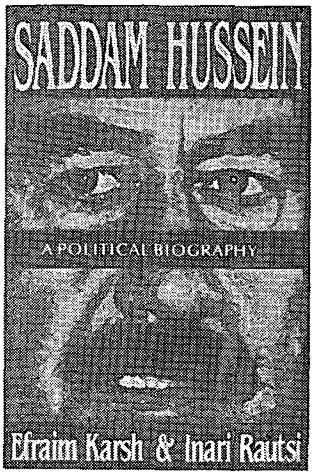

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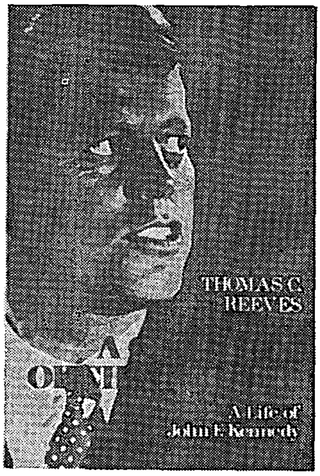

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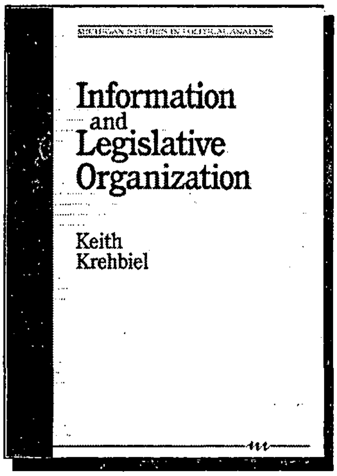

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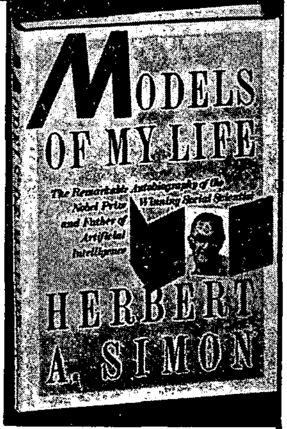
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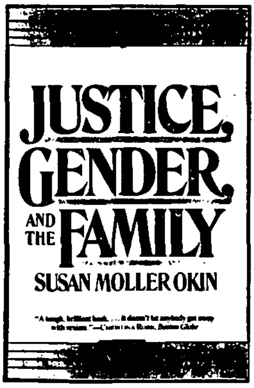

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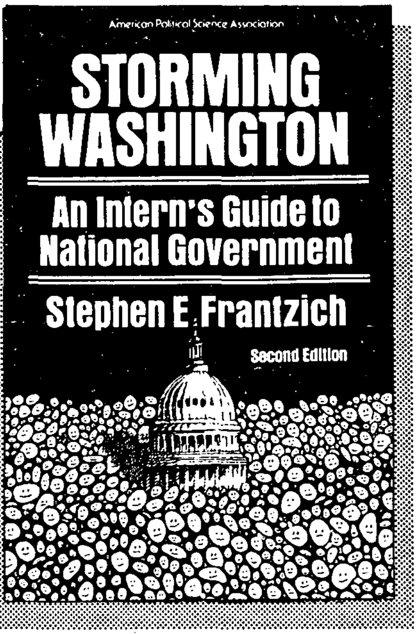
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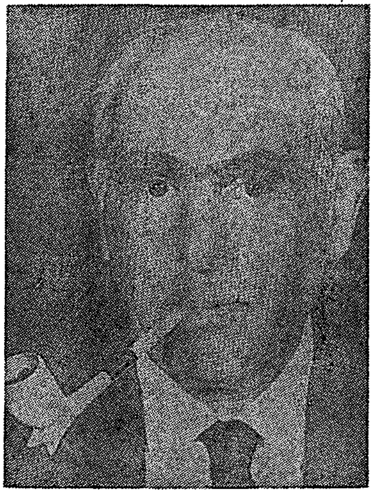

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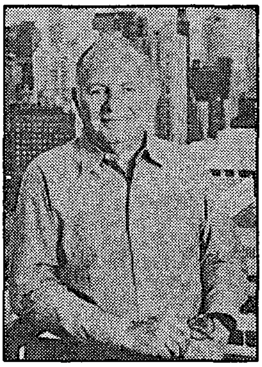

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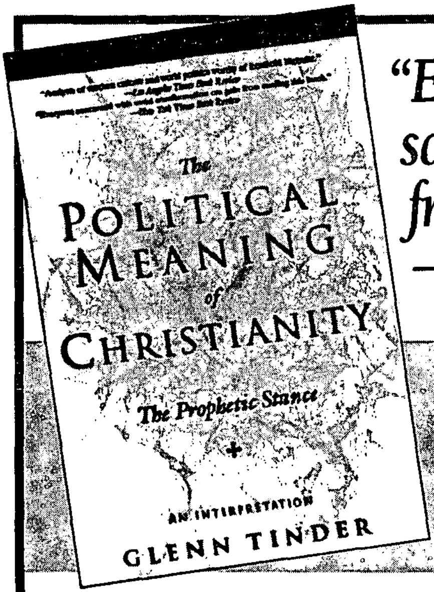

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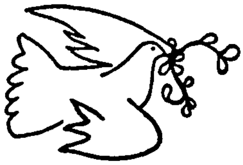

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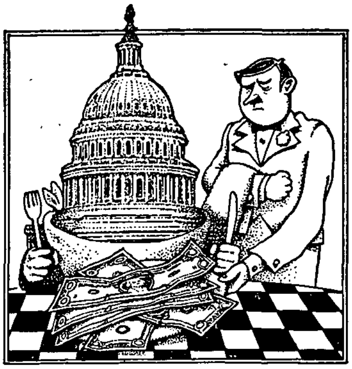

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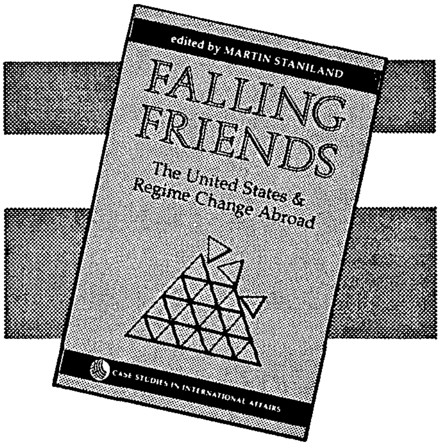
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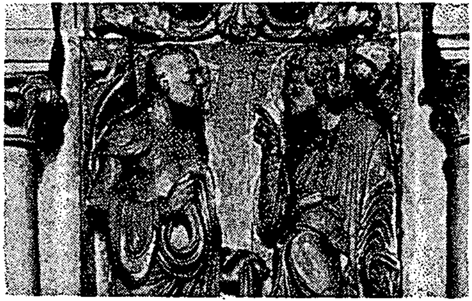

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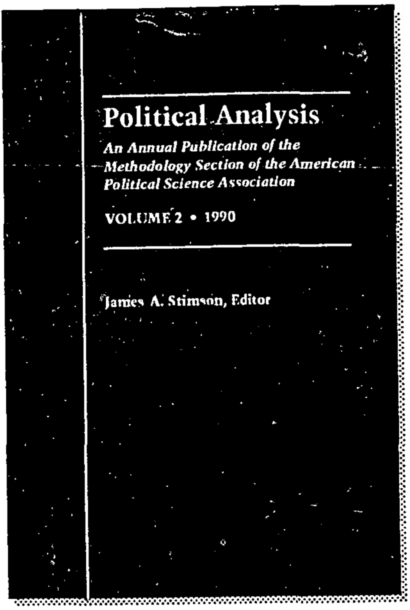

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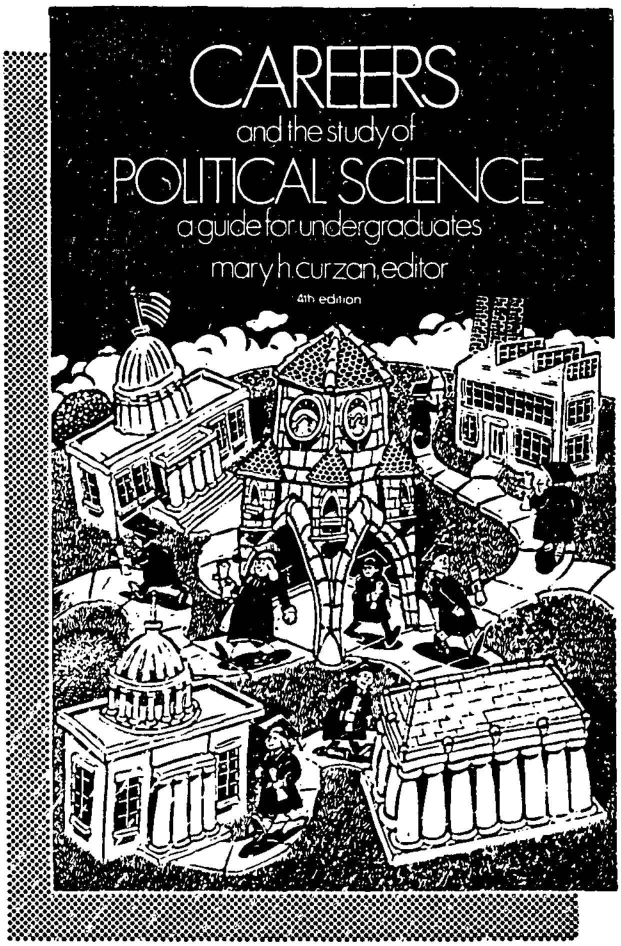
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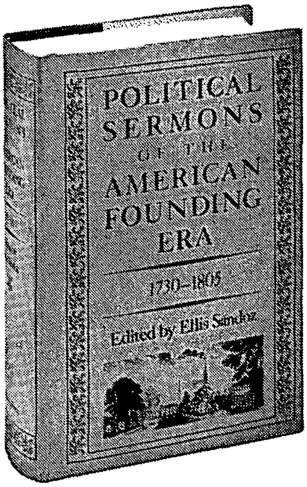

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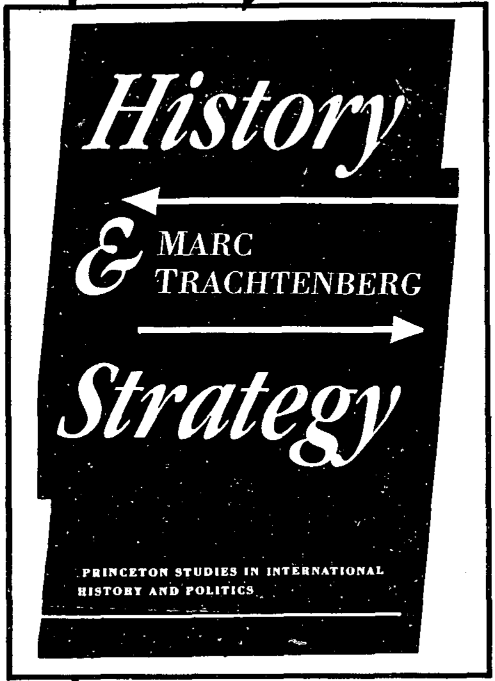

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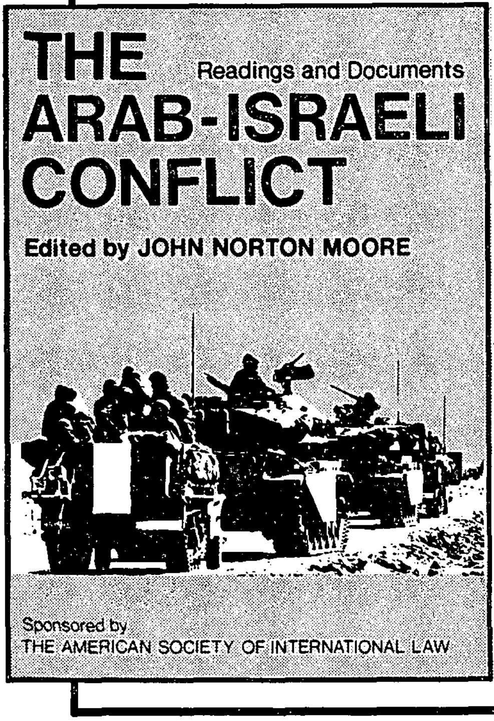

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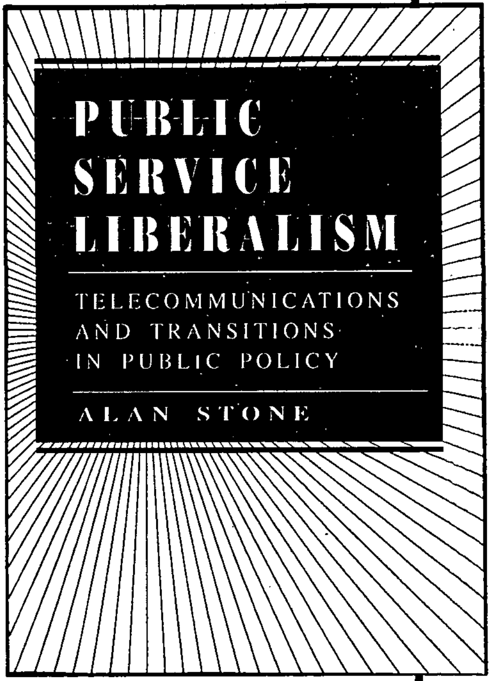

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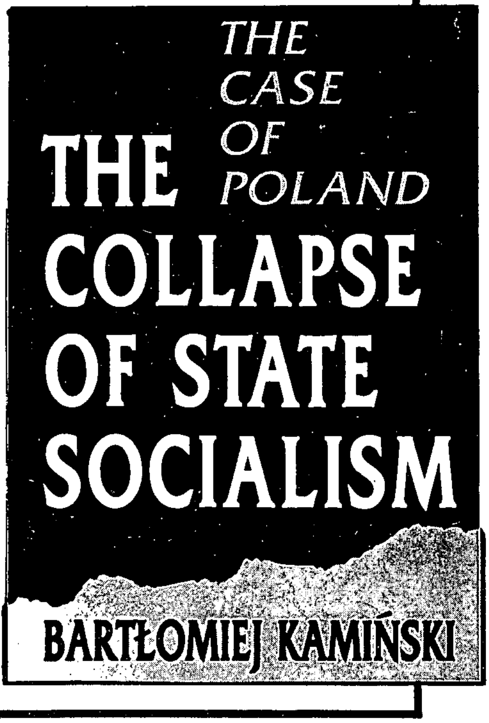

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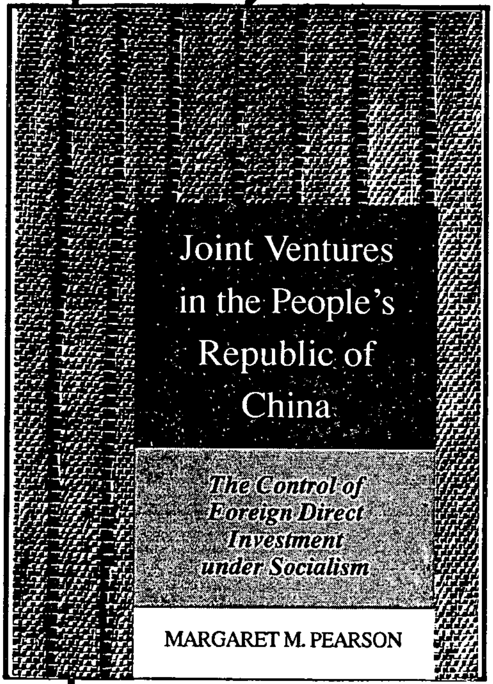
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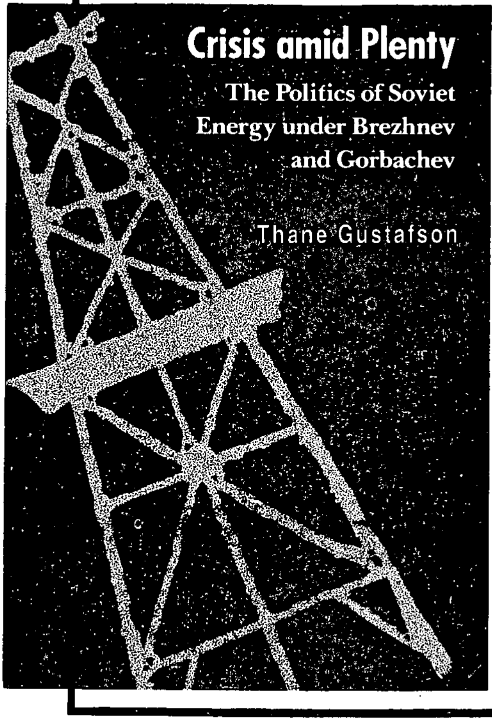
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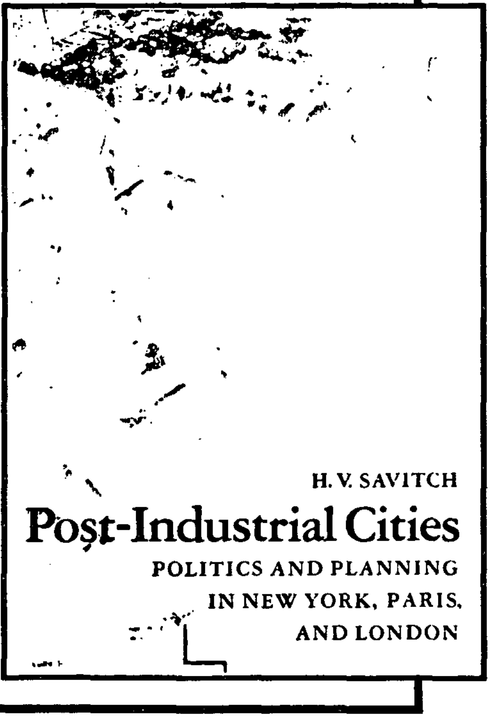

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